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ART. XVIII.—*Some points in Botanical Nomenclature; a Review of "Nouvelles Remarques sur la Nomenclature Botanique,* par M. Alph. de Candolle," Geneva, 1883; by ASA GRAY.

SIXTEEN years have passed since M. de Candolle laid before the International Botanical Congress held at Paris, August 16–26, 1867, a body of Laws of Botanical Nomenclature, which he had drawn up for consideration by that assembly. The code was discussed by a special committee, afterward by the Congress in full session, some modifications introduced, and it was then all but unanimously voted, "by about one hundred botanists of all countries:" "That these Laws, as adopted by this Assembly, shall be recommended as the best guide for Nomenclature in the Vegetable Kingdom." The adopted Code, with an extended Commentary, was published by DeCandolle early in the autumn of the same year; and an English Translation, made by the lamented Dr. Weddell, appeared early in 1868. The "Laws," but without the more voluminous explanatory commentary, were reprinted from the English translation in this Journal in July of that year, occupying only twelve pages; and some remarks and suggestions by the present writer were appended. As was then said, the code did not make, but rather declare, the common law of botanists. It announced principles, systematically and perspicuously, and indicated their application in leading cases; but many practical questions, as well as conflicts of rules in particular instances, which would inevitably come up, were necessarily left

to be settled when they arose. No small discussion upon certain details has in fact ensued, in which our author, naturally appealed to, has taken an active part.

In the first part of the present publication (of 79 pages, 8vo), the discussions of the intervening years are summed up and reviewed; and a few changes, which experience has shown the need of, are proposed. In the second part the author takes up certain questions which the Paris Congress left untouched, such as the nomenclature of organs (which he had treated in his recent *Phytographie*), the nomenclature of fossils, and the rules according to which names and authorities should be cited when old genera are combined or reconstituted; also some matters of orthography and punctuation are briefly considered. Finally, in a third part, the Laws adopted by the Paris Congress are reprinted, with the suggested changes. The alterations and additions are printed in italic type, so that they may be seen at a glance.

We are not sure of an English edition; and in any case it is desirable to make so important a publication as this generally known to our own naturalists. So our abstract and comments may run to some length. They are intended partly to illustrate and re-inforce the author's doctrine, in respect to matters upon which there is still diversity of opinion and practice, perhaps occasionally to offer a criticism or suggestion; also as helps and guides to botanists in our own country, who are beginning to take interest in such matters and to feel (or at least to show) the need of giving attention to them.

As respects all the weightier matters of the law, the most experienced phænogamous botanists are in general agreement. Having accepted the code of 1867, they will be ready to accept proposed modifications and interpretations which are in accordance with its principles. Among them all, perhaps no one is so well qualified as DeCandolle, by the bent of his mind and course of his studies, his opportunity of leisurely consideration, and his long editorial experience—combining, as it may be said to do, that of two generations,—for declaring what the present consensus of authority is, and for discussing the cases which still need adjustment, some of them of considerable consequence in phytography.

At the head of the publications upon nomenclature emanating from or sanctioned by associations or committees of naturalists, which have appeared since the year 1867, DeCandolle places the Report to the American Association for the Advancement of Science, at the Nashville meeting, 1877, on Nomenclature in Zoology and Botany, prepared by Captain Dall, after much conference with leading American naturalists. The differences between these rules and those of the botanical code

relate mainly to questions mooted by the zoologists, whose systems and views had been comparatively loose and variant. The first alteration in DeCandolle's revised draft is adopted from Dall's report, with high commendation, viz: in

"*Art.* 3. The essential point in all parts of nomenclature is 1. *fixity in names;* 2. Avoidance or rejection of forms or names which may create error or ambiguity or introduce confusion into science." The clause in italics is taken from the American code, and is said to supply a real omission. Although merely declaratory, some practical consequences flow from it. The same idea dominates in the Report made by Douville, chairman of a committee of the Geological Congress at Bologna in 1881, and which concerned itself with nomenclature in paleontology. This report insists that "The law of priority being fundamental in nomenclature, it appears to be necessary to apply it with all possible generality and to suppress derogations and exceptions to this law. . . . . Contradiction between the signification of a name and the characters of a genus or species is no sufficient reason for changing such name," etc.

Another code referred to is one by a committee of the Zoological Society of France, in 1881, M. Chaper, chairman, in which this principle of fixity is said to be less prominent, more exceptions being allowed.

On reviewing the whole field, DeCandolle assures us that the tendency during the last sixteen years has been, 1, to an increasing agreement of the zoologists with botanists, and 2, an increasing recognition of the law of priority as the fundamental principle in nomenclature, and as prevailing over considerations of elegance, linguistic purity, and precise meaning. But some recent publications of individual botanists look the other way; notably so two publications by Saint-Lager, *Réforme de la Nomenclature Botanique* and *Nouvelles Remarques sur la Nomenclature Botanique*, published at Lyons, in the Annals of the Botanical Society of that city;—of which it may be said, that the reflex effect of the multitudinous changes proposed in the view of making old and accepted names better, more classical, or more significant, has greatly strengthened the hands of those who contend for the absolute fixity and unalterableness of published names. Whether and to what extent misspelled or otherwise wrongly formed names may be corrected, and whether in some cases the principle of fixity should not prevail over absolute priority, are matters which may be discussed further on. We take up in the author's order the points which we wish to specify or to comment on.

Article 6 of the code declares that "scientific names should be in Latin. When taken from another language, a Latin ter-

mination is given to them, except in some cases sanctioned by custom." Here our author asks, "but what Latin?" He concludes that the Latin of Linnæus should be the model. It is the classical language of botany, and is much more precise than the Latin of antiquity, in which very many words bear two, three, or half a dozen senses, either in the same or in different ages; while in the technical language of botany each word has but one meaning, and each idea or object is expressed by a single term. DeCandolle elaborated this point in his Phytography, to which he refers for illustrations; and he returns to it, in special applications, when commenting on art. 66, as we shall see. But, in fact, even technical language cannot always avoid ambiguities and the use of words in senses which have to be determined by the context. *Folium*, for example, in botanical description, may mean the blade of a leaf only, or this along with its petiole, or blade with petiole and stipules together, or it may even mean any homologue of the ordinary leaf.

"Art. 15. Each natural group of plants can bear in science but one valid designation, the most ancient, whether adopted or given by Linnæus, or since Linnæus,—provided it be consistent with the essential rules of nomenclature." DeCandolle now adds an article 15$^{bis}$, which is purely explanatory, but has a bearing upon subordinate questions. It is:—"The designation of any group, by one or more names, has not for its object the enunciation of its characters or history; it gives merely the name by which we are to call it." He comments upon the tendency which is often shown to mix up the question of name with quite other considerations. Before Linnæus introduced the binomial system, the names of species were at the same time names and characters. In separating these two things, Linnæus rendered a great service, and we should be careful to preserve this advantage. "A name is a name; characters are characters; the succession of names is synonymy. To mingle such different ideas leads to confusion. In these days there is a disposition to attribute too much importance to the meaning of names, and also to intermix the synonymy—i. e. the bibliographical history of the groups—with the names, at least with the indication of the author, which being commonly annexed comes to be almost a part of the same. Such complications are contrary to the general principle that different ideas should be expressed separately. If this rule is neglected, we may be led into attempts to express in the name, or with the name, the phytogenetic history of a group, that being just now one of the ideas in vogue." Names that have an appropriate meaning are very well, and botanists always endeavor to make such; but experience shows that meaningless names are in some respects

better,—are generally better than names founded on ideas, which in the progress of knowledge often become false.*

DeCandolle's main remarks upon art. 15 relate to the point of departure for the law of priority in botany. The names of the two great classes, *Monocotyledones* and *Dicotyledones*, are of Ray in 1703, who scientifically distinguished and named them. The counterpart *Acotyledones* is of Jussieu (1789), and to all three names our author would apply the law of priority. That the name *Acotyledones* fails to express the true character is a small objection. This is only an example of the disadvantage of significant names, which may lose aptness in the advance of knowledge. Far better are such names as *Aves*, *Pisces*, *Vermes*, etc., which time and discovery can never falsify. *Cryptogamia* as the name of a class (which for meaning is hardly so good as *Acotyledones*) was introduced by Linnæus in 1735, and has maintained its place.† The counterpart, *Phanerogamia* (or *Phænogamia*), is not of Linnæus, but much later. Natural Families or Orders date from Jussieu's Genera Plantarum, in 1789. Cohorts and Tribes from A. P. DeCandolle's Systema, 1818. Subgenera begin with R. Brown, in 1810, according to our author. There are a few instances (without the name) in the Prodromus Fl. N. Holl. But it is in the Oudney and Denham-Clapperton paper, in 1826 (p. 16), that their use is discussed, and the mode of designating them in citation (by interpolation between generic and specific name in parenthesis) is introduced. Species (as distinguished from varieties) and the actual binomial nomenclature date from 1753, and the first edition of the Species Plantarum. There is substantial agreement among botanists as to this point of departure; and the fact that the specific phrase of earlier authors is occasionally of a single adjective does not militate against it. *Galega vulgaris*, *Lappa major* and *Trifolium agrarium* of the old herbalists were in good binomial form; but the adjectives are phrases, not specific names.

Generic names bring in a question of interpretation and usage. In his table, DeCandolle makes the point of departure for priority, Linnæus, Genera Plantarum, ed. 1, 1737. All agree, or should agree, that no anterior name has right of priority to a Linnean name or to a name adopted by Linnæus. But as respects generic names adopted by him, are we to follow Linnæus or are we not? He says, "*Tournefortius* primus characteres genericos ex lege artis condidit." And in the Genera Plan-

* Or may be essentially false from the beginning. One of our common Maples has two names, *Acer dasycarpum* and *A. eriocarpum*, both signifying that the fruit is woolly, whereas it is perfectly glabrous; only the ovaries are woolly. Yet no botanist has ever proposed to change the received name,—which is remarkable.

† On p. 58 DeCandolle has collected nineteen synonyms of the name *Cryptogamia*, all of later date, and specified the objections which may be brought against each of them, besides that of want of priority.

tarum: "Ipsi non immerito inventionis gloriam circa genera concedere debeam,"—and so he uniformly accredits to Tournefort the generic names adopted from him; and the same as to "*Plumerius*, . . . *Vaillantius*, *Dillenius*, . . . . *Michelius* et pauci alii," "qui ejus vestigia presserunt." DeCandolle remarks that Tournefort had the merit which Linnæus ascribes, but that "he kept a good many adjective names for genera (*Acetosa*, *Bermudiana*, etc.)." Since Linnæus did not adopt these, they are out of the present question. Moreover, not to speak here of a score or two of really adjective generic names, Linnæus himself adopted two which Tournefort had discarded, *Mirabilis* and *Impatiens*, and deliberately made another, *Gloriosa*, in place of a proper name, *Methonica*, of a sort which, though not of the best, is now regarded as next to the best. But it is completely understood that Linnæus is not to be corrected; so *Gloriosa*, *Impatiens*, etc., remain. Are we equally to follow Linnæus in regard to names which he adopted from Tournefort and a few later authors, some of them his own contemporaries? If so, we shall continue to write *Salicornia* Tourn., *Corispermum* A. Juss., *Olea* Tourn., *Justicia* Houst., *Dianthera* Gronov., *Lycopus* Tourn., *Linnæa* Gronov. The practice of the leading botanists has been essentially uniform in this respect, from Jussieu down to DeCandolle, father and son, even to the latest volume of the Monographia, published during the current year. It seems perfectly clear, therefore—although we believe the question is not raised in this revision—that such genera are expected still to be cited as of their respective founders. And, as hardly any one doubts that Tournefortian genera suppressed by Linnæus but restored by modern botanists (such as *Fagopyrum*) are to be cited "Tourn.," it follows that only in a restricted sense do genera begin with Linnæus in the year 1737. This case, indeed, is governed by the principle in citation, so well insisted on by M. de Candolle, that no author is to be represented as saying the thing that he does not say. The alternative course is to write "*Linnæa* Linn., which is certainly what Linnæus has not said. The only authors we know of who have on principle followed this alternative—and a notable exception it is—are Bentham and Hooker in the new Genera Plantarum, against which a protest was made in this Journal when the first part was issued. They have followed the rule that botanical genera began with Linnæus so strictly as to cite even authors as recent as Gærtner for Tournefortian genera, and to ignore botanists like Gronovius, contemporary with Linnæus, and publishing since the year 1737; and it is only by an infraction of their rule that they have avoided writing *Linnæa* Linn.

No change of rule 15 seems actually required to bring it into unison with the almost-universal practice in citation. We have

only to understand that genera adopted by Linnæus from Tournefort, etc., and so accredited, should continue to be thus cited; that the date 1737 (Linn. Genera, ed. 1), is, indeed, the point of departure from which to reckon priority, yet that botanical genera began with Tournefort; so that Tournefortian genera which are accepted date from the year 1700. That is the limit fixed by Linnæus, and it definitely excludes the herbalists and the ancients, whose writings may be consulted for historical elucidation, but not as authority for names.

Upon articles 21 and 22, which give rules for the names of orders and other supra-generic groups, our author offers no new remarks. We venture to offer two. It being the general rule that *aceæ* is the proper termination for ordinal names which take their appellation from a typical genus, it is desirable to conform to it as fully as well may be.* Since *Saxifragaceæ*, *Myrsineaceæ*, *Styracaceæ*, *Gentinaceæ*, *Nyctaginaceæ*, and even *Lauraceæ* and *Juglandaceæ* were adopted in the Prodromus, it seems to us retrograde and unadvisable to have gone back in the new Genera Plantarum to *Saxifrageæ*, and the like; and this upon no obvious principle, as we have *Samydaceæ*, *Cornaceæ*, etc., brought into harmony with the rule. And if in the Prodromus we have *Styracaceæ*, why not also *Salicaceæ?* And if, in the Genera Plantarum, *Styrax* could take the *aceæ* termination as *Styraceæ*, why not as *Styracaceæ?* If it be objected that some such terminations have an unclassical aspect, this objection applies all the more to the cases under our second remark: Namely, that too rigid adherence to the rule that names of suborders, tribes, etc., shall end in *-eæ* and the like, gives us nearly unpronounceable words of four or five consecutive vowels, or, when the diphthongs are printed in separate letters, according to a prevalent fashion, one or two more. Of these—the dipththongs written out—*Sauraujeae*, *Spiraeeae*, *Catesbaeeae*, *Jaumeeae*, *Thymeleeae*, and *Moraeeae* are the worst instances, and would justify any infraction of rules.† The last and one of the worst would have been avoided by writing the ordinal name *Iridaceæ*, when that of the tribe would have been *Irideæ*. Names are to be spoken as well as read, and botanists who

* *Cruciferæ*, *Leguminosæ*, *Umbelliferæ*, *Compositæ*, *Labiatæ*, and the like, are no exception to the rule, rightly stated, as they are not named from typical genera. We shall not have any more of them, but the old ones in use are among the best.

† Far better to write *Spiræaceæ*, with DeCandolle. The use of this termination for tribal names need not be objected to by those who take little pains to use it for orders. And those of us who are careful so to employ it, would prefer its occasional use for tribes and suborders to the concatenation of vowels, which it is not easy to write and almost impossible to pronounce. Some quite unnecessary tribal names in *aceæ*, such as *Vernoniaceæ* and *Eupatoriaceæ*, adopted by DeCandolle from Lessing, are kept up, although exceptional.

have to teach think more of these things than those who only write.

At the head of his remarks upon generic names (art. 25, et seq.), our author commends to other naturalists the very clear directions given in the rules for Zoological Nomenclature, edited by Dall, for rendering Greek letters into Latin in the construction of generic and specific names. He notes, however, that the rendering of $\eta$ by *e* is not in full accordance with Latin usage, as witness *bibliotheca*, *dialectica*, *Hecubea*, etc.

Art. 28 of the code, which is reprinted without alteration or comment, consists of a series of recommendations of points to be attended to in the construction of generic names. As such names are to be "in Latin," it would have been well to recommend that in their formation from the Greek, from which most of them are now-a-days drawn, the principles of Latin prosody should not be wholly ignored. Such names as *Trichóclădus* and *Ancistróclădus* would have been euphonious as *Trichocladia* and *Ancistrocladia*, and very little longer. *Acanthópănax*, *Didymópănax*, *Dimorphóchlămys*, *Trigonóchlămys*. *Aulacócălyx*, *Ancycócălyx*, *Pellácălyx*, *Micróchăris*, and the like, are harder to pronounce than the makers probably thought; and so of many others.

One of the actual recommendations is "Éviter les noms adjectifs." This in Weddell's version is translated, "To avoid adjective nouns:" doubtless a wrong translation. Adjective nouns we take to be substantives which are directly formed from adjectives. Not many such are likely to be made for genera; but if such good ones can be constructed as those we already have in *Nigella*, *Amarella*, *Flaveria*, *Chlora*, *Rubia*, *Leucas* and *Hyptis*, they will not be objected to. Clearly the recommendation is to avoid *adjective names* for genera. That may be done for the future, but has not been done in the past. They are contrary to the rule of Linnæus, but not to his practice. Not to refer again to *Gloriosa*, *Mirabilis*, and *Impatiens*, at least two score of obviously adjective names for genera may be counted in the first edition of the Genera Plantarum,—such as *Arenaria*, *Stellaria*, *Utricularia*, *Dentaria*, *Asperula*, *Angelica*, *Trientalis*, *Pedicularis*, *Digitalis*, and from the Greek such as *Polycarpon*. *Amphicarpum* and *Mitracarpum*, are recent names of this kind. To conform the rule to the fact it were better to state that: generic names are either substantives or adjectives which may be used as substantives, the latter, mostly feminine in gender. *Angelica* is understood to be *Planta Angelica*, *Sanguinaria*, *Planta sanguinaria*, etc.

It is recommended "To avoid making choice of names used in zoology. But it has become nearly impossible to follow this advice, nor is it now thought to be important.

Article 33 is suppressed. It was no more than a statement of the custom that personal names for species were to be nouns in the genitive (e. g. *Clusii*) when the person commemorated was a discoverer, describer, or an illustrator of the species, but were in adjective form (e. g. *Clusiana*) when the name was merely complimentary. The rule sometimes worked awkwardly; for many personal names do not take kindly to latinization in the genitive form, which are sufficiently euphonious as adjectives in *-ana* or *-anum;* and it is well to do away with a needless restriction.

Art. 34, which recorded the fact that many names of species are substantives, is the subject of a few remarks, called out by the publication of Saint-Lager, in which it is proposed to change all these into adjectives,—a proposition which botanists are not likely ever to adopt. Some of them are among the best of descriptive names, e. g. *Stellaria nemorum, Convolvulus sepium, Rhus vernix, Chamærops hystrix* (and such should be written without a capital initial); those which are proper names, either old names of genera or of the herbalists, are rightly said to be significant either of the absorption of a former genus, or of a transference, or as preserving a native appellation, or as indicating a likeness. "*Digitalis Sceptrum* means a *Digitalis* which had been called *Sceptrum;*" *Ardisia Pickeringia*, a species of this genus which, mistaken by Nuttall for a new one, had been named *Pickeringia; Rudbeckia Heliopsidis*, a *Rudbeckia facie Heliopsidis*, from its resemblance to a *Heliopsis*. Linnæus gave us many such names; and no sufficient reason appears either for discarding these, or for forbidding the discreet adoption of new ones. But we cannot commend such a name as *Senecio Bhot* for a species indigenous to Bhotan.

Art. 36 consists of a series of recommendations for the formation or adoption of specific names. Its fifth subarticle we may refer to in another connection, viz: along with Art. 48. The recommendation to "Name no species after one who has neither discovered, nor described, nor figured, nor studied it in any way," should be respected; yet there are occasions for departing from it, especially in case of new species in very large genera. Excellent and sometimes needful is the advice to "avoid names designating little known or very limited localities." We are obliged to cite—happily as a synonym—"*Helenium Seminariense*," published by a professor who thought he had discovered a new species of Helenium in the vicinity of the "seminary," in one of our Southern States, where he taught botany.

Article 40, suggested that names of varieties originated in cultivation, and still more half-breeds and sports (so important

for horticulturists to distinguish), should have only fancy-names, generally vernacular, and in some form as different as possible from the Latin specific names of botany,—names which, when needful, may be appended to the botanical name of the species, when that is known, e. g. "*Pelargonium zonale*, Mrs. Pollock." This has been seconded by the editor of the Gardeners' Chronicle and other judicious experts, and is slowly making its way.

Article 42, treating of the conditions of publicity, is the subject of additional remarks. The rule is, that "Publication consists in the sale or the distribution among the public of printed matter, plates, or autographs. It consists, likewise, in the sale or distribution, among the leading public collections, of numbered specimens, accompanied by printed or autograph tickets, bearing the date of the sale or distribution." DeCandolle now remarks that distribution among the members of an exchange club, of collections not offered to the public, does not come up to the rule; also that, as Dr. J. Müller states, the distribution of specimens without characters or any indication of the reason for calling it new, is nearly tantamount to announcing a species or genus in a publication, but without characters; which Art. 46 declares is not publication. But the cases are not quite alike. The possession of the named specimen enables a botanist to ascertain its distinctions. A published description without access to specimen may or may not serve the same purpose, very often does not. Unfortunately an insufficient or even a misleading description—and we have many such to deal with—claims the same right of priority that a good one does. It is well, therefore, that publication by sufficient distribution of named specimens should be recognized. But the remark is true that, in fact, very few distributed collections fulfill all the requirements of Article 42.

Article 47, sect. 2, recommends botanists "To publish no name without clearly indicating whether it is that of an order or of a tribe, of a genus or of a section, of a species or of a variety,—in short without giving an opinion as to the nature of the group to which the name is given." Unless this is attended to, and unless citations are equally precise,—for instance, unless subgeneric names are cited as such and not as generic, and *vice versa*,—much confusion in synonymy and in indexes will ensue.

Article 48, on the citation of the authority for generic and specific names, and matters therewith connected, involves questions which have been more disputed than any other. In the revised article the phrase printed in italic type is interpolated: "For the indication of the name or names of any group to be accurate and complete, *and for the ready verifica-*

*tion of the date*, it is necessary to quote the author who first published the name or combination of names."* The statement might be simplified by omitting "or names" and "or combination of names," on the ground that the name of a plant is one, that it has a name, not names, its *name* being the generic followed by the specific appellation. *Ranunculus bulbosus* is one name of two words. Our veteran botanist, Bentham, has insisted upon this; and it has a bearing upon the mooted question of mode of citation of authority.

The governing principle for citation of authorship, etc., is well declared by DeCandolle: "Never make an author say that which he does not say." It is difficult to go wrong when this principle is kept in mind, and when it is also understood that the appended name of an author, or its abbreviation, makes no part of the name of the plant, but is only the initial portion of its bibliography. Those who take a different view seem to have fallen into it by failing to distinguish strictly between name and history, and especially by mixing the history of a preceding with the statement of an actual name. A single example may illustrate this. When we write "*Mathiola tristis* Brown," we give the name of a certain kind of Stock and the original authority for it; and we may, when needful, complete the citation by adding the name of the book, with the volume and page, where it was first published. If, with some, we write "*Mathiola tristis* Linn.," we make an untrue statement. Linnæus had a wholly different genus *Mathiola*, and no *M. tristis*. If we add "sp." and somewhere explain its import to be that the latter half of the name was given by Linnæus, the other half remains unaccounted for. And we have still to seek in the synonymy for the name of the genus under which Linnæus

* M. de Candolle appears to insist upon this verification (or incipient verification) for higher groups as well as for genera and species, and would deprecate the not unusual custom in compendious floras, catalogues, etc., of omitting to cite the authority for orders, suborders, tribes, etc. We should agree with him if the omission was held to signify that the names of the groups in question, when thus simply given, were proposed as new. But in fact, authority is omitted, not because the groups are new, but because they are old and entirely familiar. Nobody will ever suppose that *Ranunculaceæ*, *Clematideæ*, etc., nakedly written, are novelties. In this regard, the nature and plan of the publication are to be considered; what is necessary in a Systema or a Genera Plantarum may be superfluous in a local or a compendious work. Indeed, it may be nearly impossible to assign the authority for the name of an order correctly, without explanation and extended references. One would wish to write, succinctly, *Cornaceæ* DC., as DeCandolle founded the order, but it was in the form *Corneæ*. A well-known order was instituted as *Onagræ* Juss., Gen. Pl., which the founder altered to *Onagrariæ*; for reasons referred to in this article we may wish to adopt the form *Onagraceæ*. As all this is most familiar matter, yet may not be correctly stated without somewhat detailed exposition, why not in a local or condensed botanical book write simply *Onagraceæ*? The proper exposition is in place in a Genera Plantarum; and it would have been better if Bentham and Hooker had critically attended to this, instead of referring merely to the preceding work of Endlicher. It would have added somewhat, yet not very much, to their great labor.

knew the plant, and also for that of the author who transferred it to *Mathiola*. If, with others, we write "*Mathiola tristis* Linn. (*Cheiranthus*)," or "*Mathiola tristis* Linn. (sub *Cheirantho*)," our longer phrase still wants the essential part of the citation. If, to secure this, we write "*Mathiola tristis* Linn. (*Cheiranthus* Brown)," our name, if it may be so called, now extended to five words and two signs in print, or of seven words when spoken, is still ambiguous and confused. It is a jumble of names, synonym, and authorities, which become explicit and clear only when we translate it into "*Mathiola tristis* Brown (*Cheiranthus tristis* Linn.)," that is, into name and synonym, with respective authorities. This is clear and literally truthful; the injection of the synonymy into the name is neither. Linnæus reformed nomenclature by freeing the name from the descriptive phrase. The school in question would deform it by rebuilding, in another way (as DeCandolle observes), ante-Linnæan phrases, only making them historical instead of descriptive.

The practice of appending the authority to the name whenever a species is mentioned has been so strictly and pedantically adhered to, that many take the former to be a part of the name. To obviate this impression, it might be well to treat the names of common plants as we do those of genera; that is, to omit the reference to authorship in cases where there is no particular need for it. Not, however, so as to cause any confusion with the cases referred to in the following paragraph:

"When a botanist proposes a new name .... it is impossible for him to cite an author; consequently the absence of such citation suffices to show that the name of the species or other group is new. Linnæus, Lamarck, DeCandolle, R. Brown, Martius, etc., followed this course. It is then a useless complication of many modern naturalists to append *mihi*, *nobis*, *sp. nov.*, *gen. nov.*, etc., to a new name. A large majority of species, genera, and families were published without these wholly personal indications." This is good as a general rule; but the *gen. nov.* and an indication of the order or tribe are often needful.

No new comments are made upon article 49, probably because the practice of botanists generally is conformed to it. The article reads, "An alteration of the constituent characters, or of the circumscription of a group, does not warrant the quotation of another author than the one that first published the name. .... When the alteration is considerable, the words *mutatis char.*, or *pro parte*, or *excl. syn.*, *excl. sp.*, etc., are added," etc. The translation would have been better worded "does not warrant the quotation of another author *in place of* the one that first published the name." For, in fact, the addition of the reforming author's name to the citation is often warranted and helpful, sometimes is almost a necessity, in the case of

genera. It appears that R. Brown began, in an oblique way, the practice objected to, and for which there is often a plausible excuse; and the elder DeCandolle sometimes followed it. It was only when the practice was systematically carried out by one or two authors, that the consequences became apparent—for few genera or species have now their Linnæan limits or signification—and the new rule was practically proved to be a necessity.

Among the recommendations contained in Article 36 was the following. "Readily adopt unpublished names found in traveler's notes or in herbaria, unless they be more or less defective." Guided by the practice of the elder DeCandolle and his contemporaries, it used to be thought a duty, or at the least a part of common courtesy to do this, in all cases in which the author's approval could fairly be supposed. But certain inconveniences and misunderstandings have resulted, especially as to mode of citation, which have suggested its withdrawal or modification. In this Revision, M. deCandolle only adds the restriction, "or unless the author has not in advance approved the publication." This does not alter the case, except for living authors: their approval ought to be obtained or counted on: and in respect to authors no longer living a botanist takes up only such names as in his opinion ought to be published, and which he supposes the posthumous author would have approved. On the whole, it were probably better not to take up names left unpublished by a deceased botanist; and DeCandolle assigns good reasons for letting them alone. If he had modified the article decidedly in this sense he would have more fully expressed his own view, and probably have been sustained by prevalent opinion. Moreover, he might have distinguished this practice from an essentially different one, viz:—

The case of plants sent under manuscript names by a discoverer or an investigator to some botanist engaged in publication, and with a view to their publication;—of which the sending by Nuttall of new *Umbelliferæ* to the elder DeCandolle when elaborating that order for the Prodromus, is a marked and not unusual instance. For this is a practice that need not be discouraged. Any small inconvenience that may arise as to mode of citation is counterbalanced by the greater concentration in publication, new genera and species thus appearing in monographs, floras, or in the papers of leading botanists, which otherwise would have dimly seen the light in obscure or local periodicals. And they are more likely to have proper characters assigned to them, instead of vague descriptions, by incompetent or unpracticed hands, such as often try a botanist's patience.

Article 50 treats of the mode of dealing with such names as

the above-mentioned after they have been published, i. e. "names published from a private document, an herbarium, a non-distributed collection, etc." It declares that such names "are individualized (Fr. *precisés*) by the addition of the name of the author who publishes them, notwithstanding the contrary indication that he may have given." This is found to mean that, although the elder DeCandolle gives us "*Eulophus* Nutt.," as the name of a genus communicated by Nuttall, with a specimen, for the purpose of its being so published in the fourth volume of the Prodromus, yet subsequent writers, looking only to the work it was published in, are to cite it as *Eulophus* DC. And that the genus which Linnæus published as "Linnæa, authore Clariss. Dr. Gronovio," we are to cite as *Linnæa* Linn. This is not only quite contrary to the regular practice of botanists from Linnæus down to DeCandolle and later, but is also contrary to the golden rule of citation, already referred to, never to make an author say something different from or opposed to that which he does say.

Appreciating this, the author of the code has now recast Article 50, so as to read, "When an inedited name has been published [by another botanist], in attributing it to its author, those who afterwards mention it ought to add the name of the person who published it; for example, *Leptocaulis* Nutt. in DC.; *Oxalis lineata* Gillies in Hook."

This is reasonable, and in the first instance such names will almost of necessity be so cited, must always be so cited when work, volume, etc., are specified. But, DeCandolle remarks that the addition will soon vanish, for instance, that the "*Cynoglosum ciliatum* Douglas, Mss.," published by Lehmann in Pugillus, etc., and in Hooker's Flora Boreali Americana, will soon come to be quoted simply as "*Cynoglossum ciliatum* Dougl.," that is, just as other names are quoted. And why not? Because, it is said, the name dating only from the publication, it is necessary to know when and where this vicarious publication was effected. For this "Nutt. in DC." may fairly serve, nearly all names published by DeCandolle being contained in the Prodromus. Not so, however, with "Gillies in Hook." Sir Wm. Hooker published very widely, in periodicals, in the Botanical Magazine, and in numerous independent works. In such cases the double citation gives little help. The experienced botanist may know where to look; the inexperienced must turn to indexes at once; for both these must be the final and the usual resort; and in them the double has little if any advantage over the single citation. Moreover, if this principle is fully applied, the number of double-cited names may be inconveniently numerous. The first volume of Torrey and Gray's Flora of North America abounds in species and genera

published by them for Nuttall. If these have all to be permanently quoted "Nutt. in Torr. & Gray," why not also the many species published, say by Bentham in DeCandolle's Prodromus, in the Flora Brasiliensis, etc., and even the species published by Brown in the second edition of the Hortus Kewensis, and elsewhere? On the whole it seems probable that these double citations will be used only in first or in early quotations, or in special instances; that it will not be deemed necessary to retain them when the names become settled in Floras or general works, except in the bibliography or full reference; when, of course the "*Leptocaulis inermis* Nutt., in DC. Coll. Mem. v. 39, x. 10, et Prodr. v. 107," will fully appear. But so long as the abbreviated citation of author and publisher together is requisite, the mode of citation recommended by DeCandolle is the one to be employed.

A quite different case is that of citing, as authority for a genus or species, the name of a botanist which is not upon the record. There is reason to believe that L. C. Richard edited the Flora Boreali-Americana of Michaux, and drew up the excellent generic and specific characters of the new plants in it. There is equal reason to believe that he purposely withheld his name. Upon no just principle of citation, therefore, can the name of Richard be quoted, as the younger Richard and Kunth essayed to do. The same holds for the work of Solander in the first edition of Aiton's Hortus Kewensis. And if it does not hold for the contributions of Brown to the second edition, it is because he claimed them in his lifetime, rather than because they have been collected and republished under his name since his death. Only confusion will come from the admission of hypothetical constructive authorship. The old rule that, what does not appear is no better than non-existent, must apply to all such cases.

In the comments upon article 52, the duty of abbreviating authors' names in the normal way is insisted on, and the bad practice of doing so by leaving out the vowels is deprecated. *Michx.* for Michaux, which is partially shortened in this way, was a necessity on account of the ancient botanist Micheli. But *Crn.* for *Crouan* is intolerable. Such a name need not be abbreviated at all. Monosyllabic names should rarely if ever be curtailed. *R. Br.* has so long been used for Robert Brown that it may continue to be used, although *Brown* is better. In the other form, it may be counted among the few cases in which initial letters are used instead of the first syllable and first consonant of the second,—cases which should probably be restricted to the *L.* for Linnæus, *DC.* for DeCandolle, *H.B.K.* for Humboldt, Bonpland, and Kunth. We are not sure that DeCandolle would favor the latter.

A series of remarks is made upon Articles 29–66, taken together,—relating to names which are to be rejected or modified, and those which are to be maintained notwithstanding certain faults. As already mentioned, the tendency among working naturalists is to preserve names in spite of faults; while a few linguistic reformers, such especially as M. Saint-Lager, propose changes which would affect 733 recognized names of species in Europe alone, and ten or twelve thousand in the vegetable kingdom at large, and "this after all the endeavors of botanists for the past half century to establish the law of priority and to have more stability of names. *Sagittaria sagittifolia* and *Psamma arenaria* must be changed, forsooth, because they are pleonasms; all substantive specific names, because a great majority of specific names are adjectives, and many others because they are not sufficiently classical. For instance, *Dianthus* he would change to *Diosanthus*, *Mentha* to *Minthe*, *Hydrocotyle brevipes* to *H. brevipedata*, *Cactus* to *Cactos*, *Arum* to *Aron*, and so on. DeCandolle adds, that Cicero was not so particular in Latinizing Greek words, as witness *barbarus*, *machina*, *emporium*. As to *Pirus*, DeCandolle insists that even if *Pyrus* is not Latin it is the botanical name of the genus as adopted by Linnæus, [from Tournefort and from all the herbalists, and it is old enough to be entered as an alternative form in the dictionaries], and so is to be preserved under the law of priority. There is little danger that the reform of Saint-Lager will prevail. There is some danger that the reaction will so stiffen the rule of priority as to forbid the correction of obvious mistakes. See, for instance, the form in which Article 60 is now recast by DeCandolle: "A generic name should subsist just as it was made, although a purely typographical error may be corrected. The termination of a Latin specific name may be changed to bring it into accordance with its generic name." From this it would seem that a slip of the pen and a mistaken orthography of a man's name may not be corrected. We trust that, when the change would not sensibly affect the place of a name in an index, such obvious corrections as of *Wisteria* to *Wistaria* may prevail. We may assume that the error was typographical; for Dr. Wistar was at the time too well known in Philadelphia for Nuttall to have been ignorant of the orthography of the name. The correction of *Balduina* into *Baldwinia* brings it into accordance with the rule that personal names used for genera should be written as near as may be with the original orthography of the person's name. "*Astragalus aboriginorum*" is neither a typographical nor a clerical error. It is a hard rule that forbids us to write "*aboriginum*," still retaining Richardson's name as authority.

Botanists may take more kindly to the rule when applied

to such names as *Eleocharis* and *Aplopappus*, in the formation of which the Greek aspirate was neglected. We cannot well suppose this to have been a typographical or a clerical oversight on the part of Robert Brown or of Cassini. Perhaps a majority of botanical authors have preserved the original orthography, on the ground that the right of priority, like that of a certain king, is *super grammaticum*,—while the remainder have written *Heleocharis* and *Haplopappus;* whence some confusion in the indexes. The requirement to preserve the original form of generic and specific names and to abide by the Latin of Linnæus and his contemporaries, notwithstanding classical faults, enables us to retain such familiar names as *Ranunculus acris, Lathyrus palustris* and *sylvestris* (instead of *R. acer*, *L. paluster*, and *silvestris*), and to keep up *lævis* for smooth,—probably to the disgust of classical scholars.

M. DeCandolle has a note on *Diclytra* of Borckhausen, changed into *Dielytra* to make it conformable to a conjectured meaning, and then into *Dicentra* that it might agree with the etymology given by Borckhausen himself: he gives it as a case in which an excess of erudition has loaded the genus with three names in place of one; and he concludes, as do we, that it were better to have kept the original orthography, and have treated it as a name which had, through some mistake, failed of meaning. But the name having been changed into *Dicentra*, on the ground that the right word κεντρον, and not the impossible word κλυτρον, must have been intended by Borckhausen, we think it should now be maintained, although it might have been left in the original form. Moreover, the doctrine that names must not be mended and that sense is unimportant, however good and needful, is so recent that it must not be too rigidly applied to long-standing cases.

This consideration should not be wholly overlooked in the case of old and long-established genera, especially those of numerous species, for which some obscure older name has come to light. Since it is impossible to make rules for the infraction of a rule, such cases must be left to sound discretion. In our opinion such discretion would forbid the transference of the name *Stylidium* from Swartz's genus to *Marlea*, and the revival of Labillardiére's transient first *Candollea* for Swartz's *Stylidium*.

The fourth section of article 60, which enjoined the rejection "of names formed by the combination of two languages," is now suppressed. Nothing is put in its place; but let us hope that we shall not be driven to the acceptance of the specific name "*acuticarpum*" which one of our fellow-botanists has recently perpetrated. Although hybrid names are to be avoided, yet, as DeCandolle remarks, they cannot consistently

be outlawed by people who accept *centimetre*, *decimetre*, *beaurocracy*, *terminology*, and the like, nor by botanists who raise no objection to *ranunculoides*, *scirpoides*, *linnæoides*, *bauhinioides*, etc.

Names of identical meaning but of different orthography, as our author insists, may well enough co-exist. In a vast genus it might be neither inconvenient nor harmful to maintain species named respectively *fluviorum*, *fluvialis*, and *fluviatilis*, at least if they belonged to different parts of the world.

We pass to some brief annotations upon the second part of the publication before us, which deals with questions not taken up by the Congress of 1867.

The first topic is that of the nomenclature of organs, which was treated with some fulness in the *Phytographie*. The remark is here repeated that the greater part of the so-called names of organs are only *terms*, that is, names indicative of the condition of organs or parts of the plant. For some of these substantive names are necessary or highly convenient, yet most were better provided with adjective terms only, which belong to terminology, not to nomenclature. Doubtless principles of fixity and the rule of priority should apply to these, both to names and to terms. But it seems unlikely that the phytotomists will at present heed the counsels of the phytographers in this matter. Yet the latter may insist that established names used in descriptive botany shall not be displaced on the pretence of getting more appropriate ones. For instance, the long-recognized name *testa* for the outer seed-coat is to be discarded because, forsooth, this covering is not always or even not generally a shell, or of the texture of earthen ware. As well ask the French to discard the word *tête* (or *teste*], because the human head, or the skull which gave the name, does not really resemble a brick or an earthen pot.

The second is upon the nomenclature of fossils. And the rule is that they are named according to laws which apply to living plants. The Bologna congress of palæontologists ordained that, to secure priority for specific names of fossils, they should be not only described but figured. DeCandolle, after consultation with Heer (whose recent death we have to deplore), concludes that this rule is too absolute. It seems to us that so long as a large part of the names of fossil plants are merely tentative and provisional, we should be content with a general approximation to the received rules in botany.

The nomenclature of groups inferior to species (varieties, sub-varieties, variations and sub-variations) is considered; but no new rules are proposed; nor is the question of sub-species discussed.

Although it is not exactly a matter of nomenclature, we

should have liked that our author had considered the two modes of disposing of varieties, and had expressed an opinion as to whether the character of the species should or should not completely cover the variety or varieties assigned to it. In the former case there is a var. *α*, followed by *β*, etc. In the latter, the species is defined upon its type, without any special regard to the appended variety or varieties, which are then characterized as to the points in which they differ from the type. We prefer the latter method, as being on the whole clearer, and as a saving in names; avoiding the awkwardness or the superfluity of a varietal name for the type of the species.

Some noteworthy observations are introduced in respect to the plight which systematic botany is threatened with by what DeCandolle would call micromorphic botanists, like Jordan and Gandoger, who abandon the Linnæan idea of species altogether, and give this name and rank to what ordinary botanists take for sub-varieties. For example, we are informed that M. Gandoger divides the Roses of Europe and N. America into 4600 species, or groups provided with names similar to those of species, under numerous subgenera, which in effect take the place of genera. *Mentha* has already undergone a similar micro-metamorphosis. If this goes on, and the names should be written every one, I suppose that even the world itself could not contain the books (of indexes) that should be written. The obvious and only remedy is to relegate this kind of botany to a world of its own, with which the legitimate science need have nothing to do.

Questions having been raised as to the proper use of capital initials in certain specific names, M. DeCandolle has devoted two or three pages to this and related topics. Linnæus used capital initials only for substantive names; Lamarck employed them for personal and some geographical names, seemingly without system. A. P. DeCandolle used the initial capital systematically for all three, and even for *Alpina* when used to designate a plant of the Alps. His example has generally been followed until recently; and this is in accordance with the custom of the English language. To the objection that it is contrary to the customs of the Latin language, our author replies at some length, substantially as follows. He finds that in the matter of orthography, etc., classical writers distinguish nine phases or periods of the Latin language, of which the most classical is the seventh period, that of Augustus; that there is no foundation in classical Latin for either punctuation (the points distinguishing words, not phrases), or accentuation by signs, and that the distinction between capitals and small letters was made since the dark ages, by scholars whom a purist of our day might tax with ignorance of the proper way of writing Latin; that the

object and result of all these and other innovations was greater clearness and precision; that the question is not at all one of ancient latinity, but of modern usages, both of the philologists and the naturalists; and that these have happily modified classical Latin into a medium of greater precision and clearness and better adapted to the needs of science.

Finally, we have a brief discussion of the question: "When an author has comprehended one genus in another without naming the species, can he be cited for the names of the species which implicitly result from this union?" The answer he decisively gives is: "This would be neither right, nor possible, nor practically convenient. To be correct, one should attribute to an author exactly what he has published. When it is said that the genus B should be united with the genus A, this is not saying that the species of the genus B should be called by such and such names in the genus A. To name them correctly it is necessary to examine them one by one. A glance at the Genera of Bentham and Hooker, or at the works of Baillon, will show how impossible it is to attribute the designation of the species to the authors who have changed the names of the genera, without an explanation under each species." This is illustrated by the supposed case of three genera, combined into one, each of which has a species *lanceolata;* by the case of a species *minor* transferred to a genus of which it may be the largest species; and by reference to the state of all large and many small genera, full of obscure, misunderstood, or debatable species, the arrangement and naming of which can be effected only by patient and prolonged study. When this work has not actually been done by the reformers of genera, it should be left to monographers and the editors of Floras. If, by art. 45, "a species is not looked upon as named unless it has a generic name as well as a specific one," neither is it named unless a specific as well as a generic name is assigned to it. Besides the instances in which the old specific name is impossible under another genus, there are very many in which it would be improper or questionable, and in respect to which particular consideration is required. Between such cases and the plain ones in which implied naming could not go wrong, who is to draw the line? Perhaps it might be drawn at monotypic new genera with old specific names. But how to do even this upon recognized principles is a problem.

A fatal objection to the principle of names by implication is that all such names, if they are existent, must be indexed in the new Nomenclator Botanicus now in preparation. To transcribe under *Senecio* the specific names pertaining to all the genera which Bentham has referred to that already vast genus is no small matter, and a part of the work will prove superfluous if

—as we suppose to be the case—some of these genera, such as *Cacalia*, ought to be maintained. But that is only the beginning. A more recent author, Baillon, has reduced the genera of *Compositæ* nearly one half. For example, to *Helenium* he has referred *Gaillardia*, *Actinella*, *Cephalophora*, etc.; to *Tagetes* he has referred *Dysodia*, *Nicolettia*, *Hymenatherum* and others; to *Helianthus* a greater number of genera, most of them prolific in species. In all probability, most of these reductions will not be approved. Yet, if the principle of constructive naming is adopted, the Nomenclator must burden its columns with these hosts of inchoate specific names of Baillon, either as received names or as synonyms. It is plain that the principle referred to, besides its incongruity with the leading ideas of received nomenclature, breaks down with its own weight. There are, nevertheless, taking arguments in its favor, which need not here be recapitulated; and the common system has its disadvantages and liability to abuse. Yet it appears to be the only workable system. As already intimated, the right assignment of specific names in reconstructed genera requires particular knowledge and careful investigation. And the botanist who reconstructs genera should himself adjust and state the specific names as far as he can.

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ART. XLIX.—*Pre-Carboniferous Strata in the Grand Cañon of the Colorado, Arizona;* by CHARLES D. WALCOTT, of the U. S. Geological Survey.

DURING the month of November, 1882, the Director of the Survey had constructed, under his immediate supervision, a horse trail from the brink of a lateral cañon on the east face of the Kaibab Plateau, Arizona, down to the more level cañon bed of Nun-ko-weap Valley 3,000 feet below. Encamped in the snow, often concealed for days in the driving frozen mist and whirling snow, the party gradually overcame the apparently insurmountable obstacles in the way, and, Nov. 24th, camp was formed in the supposed "inaccessible" depths of the head of the Grand Cañon, a day of reconnoissance and rest. Then the director headed his party of faithful, energetic men and left the writer, who, through illness, had been unable to share in the building of the trail, with three men and outfit to explore and study the inner cañon valleys between the Kaibab Plateau and the Marble Cañon, and the Grand Cañon as far as could be reached south. Seventy-two days of constant work gave some of the information wished, a portion of which is used in this article.

One of the strongest topographical features of the Kaibab division of the Grand Cañon of the Colorado and the lateral cañons opening into it is formed by the basal member of the Carboniferous group, the massive Red Wall limestone of Gilbert. Breaking abruptly off at the foot of the terraced slope of the Lower Aubrey sandstone, a sheer cliff of from 800 feet at the head of the Grand Cañon, to 1,000 feet at the mouth of the Kanab Cañon, is entirely formed of this limestone. Frequently several hundred feet are added to the cliff by the abruptly breaking away of the thin Devonian series and the massive Upper Tonto (Cambrian) beds beneath.

In the field work of 1879 a line of demarcation was established at the base of the Red Wall limestone, and the presence of the Devonian between it and the Tonto group of Gilbert definitely determined on both stratigraphic and paleontologic evidence. A plane of unconformity by erosion, not dip, was found between the Carboniferous and Devonian, and also a strongly marked fauna compared with that of the Tonto beneath and Carboniferous above.*

This horizon was traced, during the winter of 1882–3, from the most northern exposure in Marble Cañon, where the base of the Red Wall limestone first rises above the river-bed, to a point in the Grand Cañon south of Vishnu's Temple. From the known structure of the cañon walls, this line is undoubtedly present the entire length of the cañon to its termination at the Grand Wash. It is known in and at the mouth of the Kanab Cañon, and Mr. Gilbert's section indicates its presence at the mouth of the Grand Cañon.

In places the Devonian is entirely absent, either through erosion or non-deposition, so that the Red Wall limestone rests directly on the massive calciferous strata of the Upper Tonto. The line of unconformity is slight and often none exists except to the eye of the geologist looking at that exact horizon for it.

The Devonian rarely has a thickness of more than 100 feet. When present it is unmistakably marked by the thin purplish colored layers of fine-grained sandstone that pass into calciferous sandrock and limestone in which Cyathophylloid corals, casts of Brachiopods and Gasteropods, and plates of Placoganoid fishes usually occur.

In the Kanab Cañon a strong line of erosion was observed at the base of the Devonian. Usually this is very slight and resembles the plane of erosion at the close of the Paleozoic. In the one case the hard massive limestones of the Cambrian were eroded and in others the friable, sandy shales of the Permian.

* This Journal, III, 1880, vol. xx, p. 221.

No traces of the Silurian groups have been discovered. At the head of Nun-ko-weap valley, where the Devonian is absent, species of the genera *Lingulepis* and *Crepicephalus* were found within two feet of the base of the Red Wall limestone. On examining the few imperfect fossils, collected in 1879 in the Kanab cañon, on which the strata just beneath the Devonian were referred provisionally to the Calciferous horizon, they were found to be generically identical with species from strata lower down in the Tonto that are the geologic equivalents of the Potsdam sandstone of the Mississippi valley.

The Tonto is the first of Powell's great plateau system of formations. Of Upper Cambrian age, it terminates below that great series of comformable deposits that extend upward through over 14,000 feet of strata to the Tertiary Pink Cliffs of Southern Utah. It occupies an important position in the topography of the inner portion of the Kaibab division of the cañon. By the breaking away of the central belt of shaly calcareous and sandy strata, the Red Wall cliff is kept clear of debris and undermined so as to preserve its mural face above a gently sloping terrace that extends down to the lower Tonto cliff. The latter is wonderfully persistent for a cliff not over 300 feet in height. Winding in and out, here a bold headland, there notched by a narrow, profound cañon, a mural precipice in range for a mile or more, then forming a symmetrically curved point around which the contours sweep in graceful curves only to recur again at the head of a broad, open cañon, the cliff is only second to the Red Wall in its influence on the scenery and topography of the inner portion of the cañon.

The geologic age of the Tonto group is determined by the presence of numerous specimens representing the genera *Cruziana*, *Lingulepis*, *Iphidea*, *Conocephalites*, *Crepicephalus* and *Dicellocephalus* that occur at various horizons in the upper 700 feet of sandstone, shales and limestone. No fossils were obtained from the coarse sandstone forming the lower 300 feet. The fauna above, however, shows close relations to that of the Potsdam sandstone horizon of Central Nevada, the Mississippi valley and Saratoga county,* New York.

The base line of the Tonto is quite uniform and rests unconformably on the varied strata beneath; here and there a knoll, point or ridge is seen on the Pre-Tonto surface that rises nearly through the massive Tonto sandstones that were deposited against and over them, the sea breaking off, and burying with the drifting sand, fragments of the rocky islands.

* The Potsdam fauna of Saratoga county occurs in a massive bedded magnesian limestone and will be described later. The genera *Ptychaspis*, *Crepicephalus*, *Dicellocephalus*, etc., are well represented.

The great unconformity beneath the Tonto has been described by Professor J. W. Powell, who examined it in his boat trips down through the Grand Cañon, and by Captain C. E. Dutton, who viewed it from the summit of the Kaibab Plateau, five miles away. Professor Powell estimated the strata beneath the Tonto and above the Archean at 10,000 feet, and as this is all cut across the unconformity is very great. A detailed study adds to the thickness of the strata; it shows that the original summit of the Pre-Tonto group had undoubtedly been cut away more or less before the deposition of the Tonto group; that the plane of erosion cut deeply into the Archean, and that besides the 13,000 feet of strata, that have been planed off, of which the record is found in the section preserved, there was also a problematical amount of considerable thickness.

Professor Powell called the Pre-Tonto group the Grand Cañon group. It is found convenient on stratigraphic grounds to divide this into two groups, a line of unconformity by erosion occurring between them and also a decided change in the character of the sediments. The lower, the Grand Cañon group, is made up of an immense mass of sandstones and interbedded greenstones exposed directly on the Grand Cañon, and the upper, the Chuar group (a name given by Professor Powell), is a series of sandy and clay shales in the interbedded sandstones and limestones that are exposed in the inner cañon valleys between the Kaibab Plateau and the six great buttes forming the west side of the lower portion of Marble Cañon.

The summit of the Chuar group is, as now known, in a little synclinal on the divide between Nun-ko-weap and Kwa-gunt valleys.*

At first a rough sandstone, it gives way below to sandy and thin argillaceous shales with interbedded sandstones and limestones, 285 feet of limestone occurring in 5,170 feet of sediments.

The summit of the Grand Cañon group is a massive magnesian limestone, 100 feet, in thickness, that overlies red sandstones resting on a massive belt of greenstones 1,000 to 1,300 feet in thickness; this belt is broken up into eight principal flows by partings of sandstone deposited between the flows. The first coulée flowed over the level ocean bed in which 5,000 feet of sediment that now forms a reddish-brown sandstone had accumulated on the upturned and eroded edges of the Archean, the few layers of limestone and the one flow of lava, 150 feet in thickness near the base, scarcely serving to break the great sandstone series. The Archean, where the section terminates, consists of thin-bedded quartzites broken by intrusive veins of

* For position of these valleys see Atlas of Dutton's Tertiary History of the Grand Cañon, 1882.

a flesh-colored granite, the layers of quartzite standing nearly vertical.

The Pre-Tonto series is a remarkable one considering its geologic age. In the Chuar group limestones and shales succeed each other with lithologic characters similar to the Trenton limestone and Utica shale. The parti-colored shales, in one belt 700 feet thick, recall the friable Permian clays. In fact there is no more evidence of metamorphism throughout the 12,000 feet of conformable beds than there is in the evenly bedded strata of the Trias and Cretaceous groups of Southern Utah. Ripple marks and mud cracks abound in many horizons, but not a trace of a fucoid or a molluscan or annelid trail was observed. But for the discovery of a small Discinoid shell, a couple of specimens of a Pteropod allied to *Hyolithes triangularis* and an obscure Stromatopora-like group of forms, the two and one-half months' search for fossils in these groups would have been without result. They serve, however, to retain the group within the Cambrian and also point to a fauna that must be searched for elsewhere, as they alone could scarcely have been the only representatives of the life in the sea at that time.

As now known, the Grand Cañon and Chuar groups may be referred to the lower Cambrian. Their stratigraphic position is essentially the same as that of the Keweenawan group of Wisconsin. Both series were originally deposited over the underlying Archean unconformably. Both were subsequently elevated, eroded, and buried beneath sediments that in each case uncomformably overlie them and also contain a fauna strikingly similar.

Professor Chamberlain, when speaking of the interval between the elevation of the Keweenawan series and the deposition of the Potsdam, suggests that, in part, at least, the Acadian period of the Atlantic border represents a portion of the work of the interval.*

With relation to the interval between the elevation of the Grand Cañon groups and the deposition of the Tonto there is a section in the Eureka District of Central Nevada which passes from the Tonto or Potsdam horizon down through 3,000 feet of conformably bedded limestones before reaching the *Olenellus* horizon. The latter in turn is underlaid conformably by a great belt of quartzite as far down as the section was exposed. These beds appear to have been those deposited in the Nevada sea during the period of land surface over the Grand Cañon area, and if the Keweenawan series correspond to the Grand Cañon group in time, as they certainly appear to do in geologic position, we would consider the interval of erosion before the

* Geology of Wisconsin, vol. i, p. 94, 1883.

Potsdam as the time of the deposition of the great belt of strata represented by the Pre-Potsdam *Olenellus* horizon of Nevada and Lake Champlain. The older St. Johns and Braintree horizons being correlated, in part at least, with the Grand Cañon and Keweenawan series.

In the Grand Cañon the series is unmetamorphosed, fossiliferous and but slightly disturbed. In Wisconsin the 1,000 feet of lava of the Grand Cañon group is represented by many thousand feet of volcanic rocks, and the coarse sediments were also inimical to the presence of animal life; there were, however, at times, beds of finer material accumulated in a less disturbed sea, and from these we may hope to hear of fossils being obtained.

From the facts presented, both the Grand Cañon and Keweenawan series are considered as belonging to the Cambrian, and united with the Paradoxides horizon of Braintree, Mass., and St. Johns, N. B.; the *Olenellus* horizon of Nevada, Vermont, New York, and Newfoundland, and the Potsdam series of Wisconsin, New York, Canada, etc., as forming the Cambrian age in America, as now known, a subject that will be treated more in detail at some future time. In this arrangement the Lower Magnesian limestone of Wisconsin and the Lower Calciferous of New York and Canada are viewed as passage beds between the Cambrian and Silurian or the first and second faunas of Barrande. In some instances the stratigraphy will necessitate the putting of these beds with the Cambrian and in others with the Silurian. As a whole they are referred to the Silurian as the types of the second fauna predominate very quickly after their first appearance.

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ART. L. — *Contributions to Meteorology;* by ELIAS LOOMIS, Professor of Natural Philosophy in Yale College. Nineteenth paper, with three plates.

[Read before the National Academy of Sciences, Washington, April 17, 1883.]

### *The Barometric Gradient in great storms.*

IN his Meteorological Researches for the use of the Coast Pilot, Part II. Mr. Ferrel gives the following formula for the barometric gradient,

$$G = \frac{1076{\cdot}4\ (2n\cos\psi + \nu)s\mathrm{P}}{\cos i\,(1 + {\cdot}004\,t)\quad \mathrm{P}'}. \qquad (1)$$

where G denotes the barometric gradient in millimeters per degree of a great circle, or 60 geographic miles,

$2n = \cdot 00014585.$

$\nu = \frac{s \cos i}{r}.$

$\psi$ = the polar distance of the station, or the complement of the latitude.

$i$ = the inclination of the wind's direction to the isobars.

$s$ = the velocity of the wind in meters per second.

$r$ = the distance from the center of the low area expressed in meters.

$t$ = the temperature of the air in centigrade degrees.

P = the pressure of the atmosphere in millimeters.

P′ = 760 millimeters.

Every storm affords an opportunity for testing the accuracy of this formula; but since every storm presents some peculiarities showing the effect of local disturbance for which the formula makes no provision, we cannot expect that the formula will be exactly verified in the case of any storm. If however we determine the average values of the elements of a large number of violent storms, the formula ought to represent these average values. These average values I have endeavored to obtain for the Atlantic Ocean and also for the United States. For the Atlantic Ocean I employed Hoffmeyer's charts, which embrace a period of three years from December, 1873, to November, 1876; but for the first year the observations for the Atlantic Ocean were few, and the isobars generally incomplete. From this series of charts I selected those cases in which the barometric depression was greatest, and in which the isobars were most regular. For these cases I measured with a scale and dividers the distance between the successive isobars (drawn at intervals of five millimeters) and by computation converted these measures into degrees of the meridian. Since the isobars are never entirely symmetrical on all sides, I made my measurements on that side upon which the winds were strongest and the gradients were the steepest. I recorded also the wind's velocity between the various isobars, the inclination of the wind's direction to the nearest isobar, and the state of the thermometer. I recorded the latitude of the low center, and the diameter of the first isobar, except in those cases in which its magnitude was apparently due to the lack of observations from that vicinity. These measurements extended from the lowest isobar up to the highest adjacent isobar, until the winds became feeble and the isobars irregular. The latitude of the center of high pressure was recorded; and if the isobars were tolerably regular, the diameter of the highest isobar was measured. Several of the charts show the isobar 715$^{mm}$; there are more which have the isobar 720$^{mm}$; and a large number show the isobar 725$^{mm}$.

The gradients can generally be measured satisfactorily from the lowest isobar up to the isobar 775 or 780. In order to extend the comparison still further, I selected the cases in which the barometer was highest and the isobars were tolerably symmetrical, and measured the distance between the isobars, continuing the measurements downward until the winds became feeble and the isobars irregular. For the convenience of those who may wish to examine a few of the most remarkable cases, without being compelled to search through the entire series of charts I will give the following examples:

| *For Low Barometer.* | *For High Barometer.* |
|---|---|
| 1875, Jan. 12. | 1875, Dec. 30. |
| Dec. 21. | " 31. |
| " 22. | 1876, Jan. 1. |
| 1876, Jan. 22. | " 2. |
| March 9. | " 13. |
| " 10. | |

The following table exhibits the average results for the Atlantic Ocean deduced from an examination of 81 cases treated in the manner already described.

TABLE I.—ATLANTIC OCEAN.

| Isobars. | Dist. Deg. | G. mm. | Velocity. Sc'le 1-6. | Velocity. Met. | Inclina'n. Deg. | Therm. centes. Deg. | Radius. Deg. | Radius. Myr'm. | Lat. it'e. Deg. | G′ | G″ | G‴ co.=1·8 | Gᵢᵛ | Gᵛ co.=1·547 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 715 to 720 | 1·199 | 4·17 | 3·35 | 15·90 | 25·4 | +2·07 | 2·489 | 276·6 | 58·0 | 3·12 | | 4·06 | 2·75 | 4·25 |
| 720 " 725 | 1·292 | 3·87 | 3·40 | 16·21 | 26·5 | 2·17 | 3·735 | 415·0 | 57·6 | 2·91 | | 3·78 | 2·53 | 3·91 |
| 725 " 730 | 1·348 | 3·71 | 3·39 | 16·14 | 27·9 | 2·17 | 5·055 | 561·7 | 57·2 | 2·76 | | 3·59 | 2·39 | 3·70 |
| 730 " 735 | 1·389 | 3·60 | 3·38 | 16·08 | 29·9 | 2·19 | 6·423 | 713·7 | 56·8 | 2·70 | | 3·51 | 2·30 | 3·56 |
| 735 " 740 | 1·420 | 3·52 | 3·39 | 16·14 | 31·9 | 2·34 | 7·828 | 869·8 | 56·4 | 2·70 | | 3·51 | 2·25 | 3·48 |
| 740 " 745 | 1·449 | 3·45 | 3·41 | 16·27 | 33·3 | 2·69 | 9·262 | 1029·1 | 55·9 | 2·71 | | 3·52 | 2·23 | 3·45 |
| 745 " 750 | 1·484 | 3·37 | 3·41 | 16·27 | 33·7 | 3·16 | 10·729 | 1192·1 | 55·5 | 2·69 | | 3·50 | 2·20 | 3·40 |
| 750 " 755 | 1·543 | 3·24 | 3·34 | 15·84 | 34·1 | 3·52 | 12·242 | 1360·2 | 55·0 | 2·60 | | 3·38 | 2·12 | 3·28 |
| 755 " 760 | 1·613 | 3·10 | 3·16 | 14·73 | 34·8 | 3·84 | 13·820 | 1535·5 | 54·5 | 2·40 | | 3·12 | 1·95 | 3·02 |
| | | | | | | | | | | | | co.=1·36 | | co.=1·92 |
| 760 " 765 | 1·695 | 2·95 | 2·77 | 12·69 | 36·9 | + 1·53 | 13·835 | 1537·2 | 54·0 | 1·90 | 2·01 | 2·73 | 1·61 | 3·09 |
| 765 " 770 | 1·818 | 2·75 | 2·39 | 10·95 | 40·7 | − 3·99 | 12·079 | 1342·1 | 53·4 | 1·77 | 1 87 | 2·54 | 1·42 | 2·73 |
| 770 " 775 | 1·976 | 2·53 | 2·14 | 9·80 | 44·4 | − 9·01 | 10·182 | 1131·3 | 52·9 | 1·71 | 1·81 | 2·46 | 1·29 | 2·48 |
| 775 " 780 | 2·165 | 2·31 | 1·89 | 8·67 | 47·9 | −13·65 | 8·111 | 901·2 | 52·2 | 1·64 | 1·74 | 2·37 | 1·16 | 2·23 |
| 780 " 785 | 2·370 | 2·11 | 1·70 | 7·82 | 50·6 | −18·09 | 5·844 | 648·7 | 51·5 | 1·57 | 1·68 | 2·28 | 1·07 | 2·06 |
| 785 " 790 | 2·604 | 1·92 | 1·52 | 7·02 | 52·1 | −22·40 | 3·357 | 373·0 | 50·7 | 1·42 | 1·58 | 2·15 | 0·97 | 1·86 |

Column 1st shows the isobars between which the distances were measured; column 2d shows the distance between the isobars expressed in degrees of a meridian; column 3d shows the gradient in millimeters per degree deduced from the preceding values; column 4th shows the average velocity of the winds denoted by a scale of 1 to 6. These velocities were re-

duced to their equivalent values in miles per hour by means of the table prepared under the direction of the Council of the London Meteorological Society and published in 1881. According to this table the values of the numbers employed in the English scale (0–12) are as follows:

| Force. | Miles per hour. | Mean. | Force. | Miles per hour. | Mean. | Force. | Miles per hour. | Mean. |
|---|---|---|---|---|---|---|---|---|
| 0 | 0 to 5 | 2·5 | 5 | 26 to 30 | 28·0 | 10 | 61 to 69 | 65·0 |
| 1 | 6 " 10 | 8·0 | 6 | 31 " 36 | 33·5 | 11 | 70 " 80 | 75·0 |
| 2 | 11 " 15 | 13·0 | 7 | 37 " 44 | 40·5 | 12 | Above 80 | 90·0 |
| 3 | 16 " 20 | 18·0 | 8 | 45 " 52 | 48·5 | | | |
| 4 | 21 " 25 | 23·0 | 9 | 53 " 60 | 56·5 | | | |

The average values of the numbers upon the scale 0–6 are hence determined to be as follows:

| Force. | Miles per hour. | Metres per second. | Force. | Miles per hour. | Metres per second. |
|---|---|---|---|---|---|
| 1 | 10·5 | 4·69 | 4 | 44·5 | 19·89 |
| 2 | 20·5 | 9·16 | 5 | 60·75 | 27·16 |
| 3 | 30·75 | 13·75 | 6 | 82·5 | 36·88 |

By means of this table the numbers in column 5th were deduced from those in column 4th. Column 6th shows the inclination of the winds to the nearest isobar; column 7th shows the temperature of the air in centesimal degrees; column 8th shows, for isobars less than 760$^{mm}$, the distance from the center of the low area to a point midway between the isobars mentioned in column 1st; and, for isobars above 760$^{mm}$, it shows the distance from the center of the high area. The average diameter of the first isobar about the low center was 3·78 degrees; and the average diameter of the first isobar about the high center was 4·11 degrees. The distances in column 9th are expressed in myriameters. The average latitude of the centers of low barometer employed in constructing this table is 58°·8; and the average latitude of the centers of high barometer is 49°·7. In computing the gradient for the different heights of the barometer, it was assumed that in going from the low center to the high center the change of latitude was proportional to the distance passed over. Upon this principle were computed the numbers in column 10th which represent the latitudes corresponding to the points midway between the isobars named in column 1st.

Column 11th shows the gradients computed by Ferrel's formula from the data in columns 1–10. For pressures above 760$^{mm}$, the second term of the formula was assumed to be nega-

tive. The numbers in column 12th denote the gradients computed by using only the first term of the formula. The mode of obtaining the other columns will be explained hereafter. This table readily suggests several important conclusions, but I will defer considering them until I have obtained the corresponding results for the United States.

The lower part of Plate I gives a graphic representation of the numbers in the preceding table. At the center of low pressure the barometer stands at 712$^{mm}$, and about this center the isobars are drawn at intervals corresponding to the numbers in column 2d. The isobars are continued up to the center of high pressure where the barometer stands at 792$^{mm}$. The entire range of pressure shown on the chart is 80$^{mm}$ or 3·15 English inches. The arrows represent the directions of the wind shown in column 6th. The figure next above the preceding shows the corresponding gradients from the low center to the high center. The distance from the low center to the high center is 29·31 degrees. The entire figure represents a length of 58·62 degrees, or 4,045 English miles.

In order to obtain the average elements of the violent storms of the United States, I employed the Weather Maps of the Signal Service. I have a complete set of the tri-daily weather maps from Nov., 1871, to the close of 1880; and I have also one daily map from the beginning of 1881 to the present time, making in the aggregate about 11,000 maps. The isobars on these maps show much greater irregularities than those upon Hoffmeyer's charts for the Atlantic Ocean. This difference may be ascribed in part to the greater number of observations, and the attempt to draw the isobars so as to be consistent with all the observations; but a large part of the difference appears to be due to local disturbing influences which are more numerous over a continent than over the ocean. In order to obtain suitable data for testing Ferrel's formula, we must in some way eliminate the influence of these local disturbing causes. If we should take an indiscriminate average of the gradients, wind velocities, etc., for all the storms, it would require an immense number of cases to furnish consistent results; and it is doubtful whether even these results would be as satisfactory as results derived from a small number of cases of violent storms which were less influenced by local causes. I have accordingly examined with great care my entire series of weather maps and have selected those cases in which the barometric depression was greatest and the isobars were most regular, and I have restricted my comparisons to that side of each storm upon which the winds were strongest, and the gradients were steepest. The comparisons were conducted in the manner already described for the Atlantic Ocean. For the convenience

of those who may wish to examine some of the most remarkable cases I give the following list.

| *For Low Barometer.* | | | | | | *For High Barometer.* | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1874, | Nov. | 22.3 | 1876, | Dec. | 29.3 | 1872, | Dec. | 26.2 | 1879, | Feb. | 26.1 |
| | " | 23.1 | 1878, | " | 10.2 | 1873, | Jan. | 15.1 | | March | 13.3 |
| 1875, | Jan. | 2.3 | | " | 22.1 | | " | 15.2 | | Dec. | 21.3 |
| | March | 15.2 | 1879, | Jan. | 2.3 | | " | 27.3 | | " | 22.1 |
| | Nov. | 11.1 | | " | 3.1 | | " | 28.1 | | " | 23.3 |
| | " | 17.3 | | " | 3.2 | 1875, | Nov. | 28.2 | 1880, | Jan. | 30.1 |
| | " | 29.2 | | " | 3.3 | 1876, | Nov. | 30.3 | | Feb. | 19.2 |
| | " | 29.3 | | " | 4.1 | | Dec. | 8.2 | | " | 19.3 |
| 1876, | Jan. | 9.2 | | " | 10.1 | | " | 14.1 | | " | 20.1 |
| | Feb. | 2.1 | | " | 26.2 | 1877, | Feb. | 13.1 | | " | 27.2 |
| | " | 2.2 | | Feb. | 21.1 | | " | 13.2 | | March | 11.2 |
| | " | 15.2 | | " | 21.2 | 1878, | Dec. | 24.1 | | " | 11.3 |
| | " | 25.2 | | " | 21.3 | | " | 24.2 | | Nov. | 23.3 |
| | March | 21.1 | 1880, | Feb. | 11.3 | 1879, | Jan. | 2.2 | | Dec. | 26.3 |
| | Dec. | 8.3 | | Oct. | 16.1 | | " | 3.1 | | " | 29.2 |
| | " | 9.2 | | " | 16.2 | | " | 4.1 | 1881, | Feb. | 6.1 |
| | " | 9.3 | 1882, | Jan. | 2.1 | | " | 26.1 | 1882, | Feb. | 18.1 |
| | | | | | | | Feb. | 25.3 | | Dec. | 7.1 |

The following table exhibits the average results for the United States deduced from an examination of 123 cases treated in the manner previously described. For convenience of comparison with the data for the Atlantic Ocean, the results have all been converted into French units, with the exception of the numbers in column 1st.

TABLE II.—UNITED STATES.

| Isobars. | Dist. Deg. | G. mm. | Veloc Met. | I'clina'n Deg. | Therm. centes. Deg. | Radius. Deg. | Radius. Myr'm. | $G^{i}$ | $G^{ii}$ | $G^{iii}$ co.= 1·645 | $G^{iv}$ | $G^{v}$ co.= 2·218 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 28·8 to 29·0 | 1·362 | 3·73 | 12·20 | 36·8 | −2·22 | 2·241 | 249·0 | 2·27 | | 3·73 | 1·72 | 3·81 |
| 29·0 " 29·2 | 1·511 | 3·36 | 11·80 | 38·3 | −2·06 | 3·677 | 408·5 | 1·99 | | 3·27 | 1·50 | 3·33 |
| 29·2 " 29·4 | 1·651 | 3·08 | 11·44 | 39·8 | −1·89 | 5·258 | 584·2 | 1·86 | | 3·06 | 1·39 | 3·08 |
| 29·4 " 29·6 | 1·749 | 2·90 | 11·09 | 41·0 | −1·72 | 6·958 | 773·1 | 1·78 | | 2·93 | 1·32 | 2·93 |
| 29·6 " 29·8 | 1·816 | 2·80 | 10·64 | 42·4 | −1·94 | 8·741 | 971·2 | 1·72 | | 2·83 | 1·25 | 2·77 |
| 29·8 " 30·0 | 1·867 | 2·72 | 10·28 | 43·3 | −3·22 | 10·582 | 1175·8 | 1·68 | | 2·76 | 1·20 | 2·66 |
| | | | | | | | | | | co.= 1·63 | | co.= 2·362 |
| 30·0 to 30·2 | 1·945 | 2·61 | 9·66 | 44·4 | − 6·17 | 9·586 | 1065·1 | 1·45 | 1·54 | 2·51 | 1·10 | 2·60 |
| 30·2 " 30·4 | 2·049 | 2·48 | 9·03 | 45·2 | −10·11 | 7·589 | 843·2 | 1·39 | 1·50 | 2·45 | 1·06 | 2·50 |
| 30·4 " 30·6 | 2·203 | 2·31 | 8·18 | 47·0 | −14·11 | 5·463 | 607·0 | 1·31 | 1·43 | 2·33 | ·98 | 2·31 |
| 30·6 " 30·8 | 2·427 | 2·09 | 7·20 | 49·2 | −17·39 | 3·148 | 349·8 | 1·17 | 1·35 | 2·20 | ·88 | 2·08 |

The average latitude of the centers of low barometer was 44°·7, and the average diameter of the 1st isobar about the low center was 3·12 degrees. The average latitude of the centers of high barometer was 45°·1, and the average diameter of the 1st isobar about the high center was 3·87 degrees. Since the average latitude of the centers of low and high pressure is so

nearly the same, in computing the gradients by the formula, the latitude of each center was taken to be 45°.

The upper part of Plate I gives a graphic representation of the numbers in the preceding table, and is constructed in the same manner as the lower figure on the same plate. At the center of low pressure the barometer stands at 28·7 inches; and at the center of high pressure the barometer stands at 30·9 inches, showing a range of pressure amounting to 2·2 inches. The distance from the low center to the high center is 22·075 degrees, and the entire figure represents a length of 44·15 degrees, or 3046 English miles.

Upon comparing table I with table II, the first fact which attracts our attention is that the gradients in the two cases do not differ very greatly, although the velocities of the wind are very unequal. For low barometer, the average gradient over the Atlantic Ocean exceeds the gradient over the United States only fifteen per cent, while the average velocity of the wind in the former case exceeds the velocity in the latter case by forty per cent. This fact indicates that the barometric gradient is determined not mainly by the wind which prevails at the earth's surface, but by that which prevails at an elevation such that its velocity is not sensibly affected by the resistance arising from the inequalities of the earth's surface.

The second fact which attracts our attention is that over the Atlantic Ocean, violent storms have a much greater geographical extent than they have over the United States. For the United States the average diameter of the area of low pressure (below 30 inches) in the case of violent storms is 23 degrees, or 1587 English miles. Over the Atlantic Ocean it is 29·3 degrees or 2022 miles, which is 27 per cent greater than for the United States; and since the gradients are fifteen per cent steeper, the depression of the barometer at the center of a violent storm is about 40 per cent greater over the Atlantic Ocean than over the United States. This difference is a permanent one and must be ascribed to a permanent cause. In the case of a great barometric depression, there is a powerful force in operation, near the central portions of this area, which draws in the surrounding air from a great distance. The greater resistance experienced by the air in its motion over the land than over the water, may perhaps partially explain the less extent of the low area in the United States. But we find that in the interior of Europe violent storms have nearly the same geographical extent as over the Atlantic Ocean. This is shown by Hoffmeyer's Weather Charts and by the International Weather Maps. Over the western portion of Europe, near the Atlantic Ocean, the barometer frequently falls as low as 720$^{mm}$, and sometimes it falls even lower than this at a great distance from

the ocean. In 1880, Feb. 29th, in lat. 61¼° N. and long. 25° E. the barometer fell to 719$^{mm}$, and in 1881, March 19th, in lat. 64° N. and long. 39½° E. it fell to 709$^{mm}$. The last storm was remarkable for an increase of intensity in its progress eastward over the continent. When it left the Atlantic Ocean the pressure at the center of the storm was 728$^{mm}$, and the pressure fell to 709$^{mm}$ (27·9 inches) as the storm advanced eastward. Over the United States the barometer never falls so low. In my eighth paper I gave a list of all the cases in which, at any of the Signal Service stations, the barometer fell as low as 29·25 inches during a period of two years. This list shows only twenty-eight cases in which the barometer fell below 29·00 inches; it shows sixteen cases in which the barometer fell below 28·9 inches; it shows five cases in which the barometer fell below 28·8 inches, and only three cases in which it fell below 28·7 inches. These three cases occurred Nov. 18, 1873, at the first, second and third observations for that day, the lowest pressure being 28·47 inches at Eastport, Me. According to the record of Mr. R. T. Paine, the lowest point which the barometer at Boston has reached during the past sixty years, is 28·47 inches (Nov. 25, 1846), and this is probably as low as the barometer has fallen in that part of New England at any time since reliable barometric observations have been made. There is no doubt that the geographical extent of areas of low pressure is considerably less in the United States than it is in Northern Europe, and I think the cause is to be found in the permanent ridge of high pressure prevailing in the southern portion of the United States. My twelfth paper was accompanied by a chart showing that during the colder months of the year there is a ridge of high pressure which is extremely persistent between the parallels of 30° and 35°. When the center of a great storm is near lat. 45°, the area of low pressure seldom extends much below the parallel of 35°. It may be inferred that if a storm center should pass farther north, the storm would attain greater dimensions and there would be a greater depression of the barometer at the center. This conclusion is certainly correct for the eastern border of the American continent, particularly for the neighborhood of Greenland; and it is probably true (though in a less degree) for the interior of the continent; but on account of the lack of observations we cannot make a very satisfactory comparison.

The column marked G′ in table I was computed from Ferrel's formula, assuming that for pressures above 760$^{mm}$, the second term of the formula should be taken with a negative sign. A careful examination of this table will show that for pressures above 760$^{mm}$ the second term should be omitted entirely. It is evident that if the circulation of the air about

a center of high pressure gives rise to a centrifugal force, its effect must be to diminish the gradient. When we go from a center of low pressure to a center of high pressure, the second term of the formula must therefore change its sign on passing the line of mean pressure. This must cause an abrupt change in the computed values of the gradient, at a point where we pass from the low area to the high area. Such a change will be noticed in the values of G′ in table I. But the observed values of the gradient show no such change. The differences between the successive values of the gradient given in column third of tables I and II, show no abrupt change marking the passage over the line of mean-pressure. We therefore conclude that by the circulation of the air around a center of high pressure no appreciable centrifugal force is developed. We also conclude that when there is a decided centrifugal force in the central portion of an area of low pressure, this centrifugal force does not extend in an appreciable degree as far as the line of mean pressure. This conclusion is indicated not merely by the numbers in column third of the tables, but by the form of the isobar of $760^{mm}$ or 30 inches, which separates a low from a high area. This isobar, instead of forming an arc of a circle drawn about a center of low or high pressure, always inclines towards the form of a straight line. This fact may be verified by the examination of any weather map showing the progress of a violent storm; and it is seen on various charts accompanying my preceding papers, e. g., paper ninth, storm of April 12, 1874; paper twelth, storm of Jan. 15, 1877, and paper thirteenth, storm of April 11, 1874. The best results to be derived from Ferrel's formula are obtained by combining the numbers in the column G′ for pressures below the mean, with the numbers in the column G″ for pressures above the mean.

Over the Atlantic Ocean for pressures less than $760^{mm}$, the average gradient computed by the formula is only 77 per cent of the observed gradient; and for pressures greater than $760^{mm}$ the average gradient computed by the formula (omitting the last term) is only 73 per cent of the observed gradient. If we multiply the gradients computed for an area of low pressure by 1·3, we obtain the first nine numbers in column thirteenth of table I, and if we multiply the gradients computed for a high area by 1·36 we obtain the last six numbers in column thirteenth. We perceive that the differences between the numbers in columns G and G‴ are not very great.

Over the United States, for pressures less than 30 inches, the average gradient computed by the formula is only 61 per cent of the observed gradient, and for pressures greater than thirty inches, the average gradient computed by the formula (omitting the last term), is also about 61 per cent of the observed gra-

dient. If we multiply the gradients computed for an area of low pressure by 1·645, we obtain the first six numbers in column eleventh of table II; and if we multiply the gradients computed for a high area by 1·63 we obtain the last four numbers in column eleventh. We perceive that the differences between the numbers in columns G and G''' are less than they were in table I.

This comparison indicates that the resistance encountered by the air, even over the ocean, is considerably greater than that assumed in the formula. Having discovered that the coëfficient sec. $i$, employed in Ferrel's formula, does not adequately represent the resistance which the air near the earth's surface experiences in violent storms, I endeavored to modify the formula so as to represent the observations more accurately, and found that the following formula

$$G = \frac{\cdot 157\, v \sin.\, \varphi + 1076\cdot 4 \frac{(v \cos.\, i)^2}{r}}{(1 + \cdot 004\, t)} \cdot \frac{P}{P'} \qquad (2)$$

gave very good results when combined with a suitable coëfficient. Column fourteenth in table I, shows the numbers computed from this formula, the last six numbers being obtained by employing only the first term of the formula. By multiplying the first nine numbers by 1·547 and the last six numbers by 1·92, we obtain the numbers in column fifteenth, and we perceive that these numbers agree with the observed gradients given in column third better than the numbers computed by Ferrel's formula. In the latter case the average error of the computed gradients (disregarding their algebraic signs), is 0·12, while in the former case it is only 0·05.

Applying the same formula to table II, we obtain the numbers in column twelfth; and if we multiply the first six numbers by 2·218 and the last four numbers by 2·362, we obtain the numbers in column thirteenth, and we perceive that these numbers accord very well with the observed gradients given in column third. For both table I and table II, the average error of formula (1) (disregarding the algebraic signs) is $0\cdot088^{mm}$, while the average error of formula (2), is only $0\cdot041^{mm}$.

It may be thought desirable to ascertain how far the preceding formulas will represent the phenomena of a particular storm. For this purpose I have selected the remarkable storm of Feb. 5, 1870, of which an account has been given by Capt. Toynbee in No. 13 of the Publications of the English Meteorological Council. Besides the observations given in Captain Toynbee's Memoir, I have endeavored to obtain additional observations both for the Atlantic Ocean and the adjacent continents. For the Atlantic Ocean I have met with small success,

but for the continent of Europe there are published observations which give very complete information respecting the condition of the atmosphere on the eastern side of the area of low pressure. Nos. 1–18 of table III show (with one exception) all the observations contained in Captain Toynbee's Memoir derived from vessels navigating the Atlantic Ocean and reporting a pressure less than 30 inches on Feb. 5th at $7^h$ $35^m$ P. M., Greenwich time, this being the instant of lowest observed pressure. The observations of the vessel Carl George are rejected as being irreconcilable with the other observations. The observations for each of these vessels are shown in table III, arranged in the same manner as in tables I and II. Instead of applying the formulas to each of these observations separately, I have divided the observations into four groups, and taken the average value of the corresponding data for all the vessels included in a group, and the results are given in table V. No. 1 indicating a pressure much less than any other vessel is allowed to stand by itself and forms the first line of table V. Nos. 2–6 showing pressures between 28·0 and 28·5 inches form the second group, and the average of the observations for these five vessels is given in the second line of table V. Nos. 7–11 showing pressures between 28·5 and 29·0 inches form the third group and the averages are given in the third

TABLE III. ATLANTIC OCEAN, 1870, FEB. 5, $7^h$ $35^m$ P. M., GREENWICH TIME.

| No. | Vessel. | Lat. | Long. | Barom m. m. | Therm cent. | Wind. | | | Gradient. mm. 1°. | Inclination. | Radius Myriam. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | Direct'n | Force. | Velocity mi. p. s. | | | |
| 1 | Tarifa | 51° 3′ | 23° 59′ | 694·2 | 11°·7 | S 11° W | 10 | 29·1 | 18·0 | 12° | 76·4 |
| 2 | Weser | 49 25 | 25 8 | 715·0 | 8·9 | S 86 W | 12 | 40·2 | 8·4 | 6 | 281·1 |
| 3 | Marathon | 50 45 | 21 34 | 718·3 | 11·7 | S 11 W | 10 | 29·1 | 9·1 | 8 | 275·6 |
| 4 | City of Cork | 50 16 | 30 15 | 720·6 | 3·3 | N 34 W | 9 | 25·3 | 5·6 | −10 | 482·2 |
| 5 | Orontes | 50 47 | 20 57 | 721·3 | 11·1 | SW | 10 | 29·1 | 8·1 | −25 | 318·9 |
| 6 | Minnesota | 51 12 | 20 40 | 723·1 | .... | S 11 W | 9 | 25·3 | 7·9 | 5 | 318·9 |
| 7 | C. Antwerp | 49 45 | 28 54 | 726·9 | 10·0 | N 56 W | 10 | 29·1 | 6·1 | 10 | 415·5 |
| 8 | India | 51 53 | 33 33 | 728·7 | .... | NW | 6 | 15·0 | 3·0 | 11 | 692·2 |
| 9 | England | 51 5 | 19 0 | 729·0 | 8·9 | S 17 W | 10 | 29·1 | 6·9 | 10 | 454·4 |
| 10 | Queen | 51 4 | 17 54 | 734·0 | 11·1 | S 34 W | 9 | 25·3 | 5·3 | −10 | 541·1 |
| 11 | Nestorian | 55 13 | 14 7 | 734·8 | 9·4 | S 28 E | 9 | 25·3 | 4·1 | 48 | 875·5 |
| 12 | Venezuelan | 46 3 | 29 42 | 739·1 | 7·8 | N 79 W | 9 | 25·3 | 4·1 | 7 | 794·4 |
| 13 | Helvetia | 47 12 | 38 53 | 740·1 | 2·2 | N 79 W | 9 | 25·3 | 2·5 | 25 | 1283·2 |
| 14 | Cuban | 45 58 | 22 23 | 740·6 | 7·2 | S 79 W | 9 | 25·3 | 4·8 | 0 | 709·9 |
| 15 | Westphalia | 47 5 | 39 30 | 740·6 | 2·2 | S 74 W | 9 | 25·3 | 2·5 | 48 | 1339·9 |
| 16 | Prussian | 48 11 | 41 31 | 743·2 | 0·6 | West | 7 | 18·1 | 2·5 | 48 | 1433·2 |
| 17 | Denmark | 48 37 | 36 11 | 747·0 | 0·6 | N 79 W | 7 | 18·1 | 2·8 | 26 | 1004·3 |
| 18 | Nile | 44 44 | 13 11 | 760·5 | .... | SW | 7 | 18·1 | 2·8 | 3 | 1281·0 |

line of Table V. Nos. 12–18 showing pressures between 29·0 and 30·0 inches form the fourth group and the averages are

given in the fourth line of table V. Table IV, Nos. 19–24 shows observations for the same instant from six stations in Ireland, Scotland and neighboring islands, and the averages for these stations are given in the fifth line of table V.

TABLE IV. LAND OBSERVATIONS, 1870, FEB. 5, $7^h\ 35^m$ P. M.

| No. | Station. | Lat. | Long. | Barom m. m. | Th'm cent. | Wind. Direct'n | Wind. Velocity mi. p. h. | Wind. Velocity mi. p. s. | Gradient. mm. 1°. | Inclination. | Radius Myriam. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 19 | Valencia | 51° 55′ | 10° 18′ | 749·8 | 7°·8 | S 22° E | 37 | 16·5 | 3·81 | 55° | 1063·2 |
| 20 | Armagh | 54 21 | 6 39 | 755·1 | 3·9 | SE | 12 | 5·4 | 3·81 | 72 | 1317·6 |
| 21 | Glasgow | 55 53 | 4 18 | 756·9 | 5·0 | S 34 E | 8 | 3·6 | 3·56 | 63 | 1498·7 |
| 22 | Aberdeen | 57 10 | 2 6 | 756·9 | 5·0 | S 22 E | 13 | 5·8 | 3·30 | 49 | 1657·6 |
| 23 | Skye | 57 29 | 6 34 | 752·3 | 4·4 | SE | 28 | 12·5 | 3·56 | 58 | 1384·3 |
| 24 | Thorshaven | 62 2 | 6 44 | 750·6 | 5·0 | South | 33½ | 15·0 | 2·79 | 0 | 1601·0 |

TABLE V. AVERAGE RESULTS.

| | Lat. | Barom. mm. | Therm. cent. | W. Velocity. m. p. s. | Gradient. mm. 1°. | Inclination. | Radius Myriam. | G′ | G″ c=1·32 | G‴ | G⁗ c=1·32 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Tarifa. | 51° 3′ | 694·2 | 11° 7 | 29·1 | 18·0 | 0° | 76·4 | 13·68 | 18·06 | 13·68 | 18·06 |
| 28·0–28·5 | 50 29 | 719·7 | 8 ·7 | 29·8 | 7·8 | 0 | 335·3 | 5·91 | 7·80 | 5·91 | 7·80 |
| 28·5–29·0 | 51 48 | 730·7 | 9 ·8 | 24·8 | 5·1 | 6 | 595·8 | 3·87 | 5·11 | 3·85 | 5·08 |
| 29·0–30·0 | 46 50 | 744·4 | 3 ·4 | 22·2 | 3·1 | 22 | 1121·0 | 3·12 | 4·10 | 2·85 | 3·76 |
| Land obs | | | | | | | | | | | |
| 29·5–30·0 | 56 28 | 753·6 | 5 ·2 | 9·8 | 3·5 | 50 | 1420·5 | 2·01 | 2·65 | 1·28 | 1·69 |

In three of these cases the direction of the wind as reported was *outward* from the center of the low area, and for the first six cases the average inclination of the wind to the isobars was zero. I have therefore inserted in table V the inclination for the first two lines as zero, although the wind reported by the Tarifa was inclined 12° inward. Column 9th in table V shows the gradients computed by formula (1) and column 10th shows the values in column 9th multiplied by 1·32. Column 11th shows the gradient computed by formula (2), and column 12th shows the preceding values multiplied by 1·32. It will be observed that both formulas give the same result in the first two lines, as should be the case since the formulas become identical when $i=0$. For the first three lines the computed gradients (multiplied by 1·32) agree well with the observed gradients; but for the fourth line the computed gradients are too great, and for the fifth line they are too small. The wind velocities reported by the vessels 12–18 are greater than are usually

found associated with the given gradients; and the velocities at the stations 20–22 were less than are usually associated with the gradients existing at that time. This example shows how a general formula (although founded on correct principles) may fail to represent accurately the gradients for the separate observations of any particular storm. The close agreement of the computed gradients with the observed gradients in the first three lines of the table must be regarded as highly satisfactory; and the discrepancies found in lines 4 and 5 show that a close agreement between the observed and computed gradients is to be expected only when the data compared are deduced from the average of a large number of observations.

The accompanying map shows the isobars for the storm here investigated. The curves were first drawn with great care upon a map whose scale was more than three times as large as that of the map here given. For the southern half of the low area the position of the isobars is well determined. For the northern half of the low area the number of observations is small, and the exact position of the isobars cannot be certainly assigned; but this uncertainty does not materially affect the conclusions derived from the observations in table III. During the progress of this storm, there was an area of high pressure prevailing over the northeast part of the United States, a second area of high pressure prevailed along the northwest coast of Africa; and a third area in the northeast part of Europe. As the pressure in the latter case was unusually high, I have collected observations sufficient to determine the position of the isobars. Table VI shows the observations from 98 stations derived from the official publications of Russia, Austria, Sweden, Norway, etc. Some of the observations were derived from the Bulletin de l'Observatoire de Paris. As these observations are only given for 8 A. M., I have taken the mean of the barometric observations for Feb. 5 and 6. Since the change of pressure during these 24 hours was generally less than $6^{mm}$ the error of the result thus obtained cannot be very great. The isobars to which the greatest importance is attached are however derived solely from observations made near 8 P. M. The barometric observations have all been reduced to sea-level. In the Russian observations the force of the wind is expressed by the scale 1–10; in the Norwegian observations the scale is 1–6; and in the Swedish observations it is 1–4.

TABLE VI. EUROPE, 1870, Feb. 5, 8 P. M., Greenwich Time.

| Station. | Lat. | Long. | Barom. sea level. | Therm. Fahr. | Direct. wind. | Force. |
|---|---|---|---|---|---|---|
| Vardo | 70° 22′ | 31° 7′ | 29·88 | 26 | W. | 4 |
| Tromso | 69 39 | 18 58 | 30·00 | 39 | S.W. | 1 |
| Bodo | 67 17 | 14 24 | 30·12 | 33 | E. | 1 |
| Haparanda | 65 51 | 24 11 | 30·40 | 14 | S. | 2 |
| Brono | 65 28 | 12 14 | 30·17 | 35 | | 0 |
| Kem | 64 57 | 34 39 | 30·60 | − 3 | S.W. | 4 |
| Archangelsk | 64 33 | 40 32 | 30·94 | −10 | S. | 6 |
| Christiansund | 63 7 | 7 45 | 30·03 | 43 | S.S.E. | 2 |
| Hernosand | 62 38 | 17 57 | 30·50 | 21 | S.S.W. | 2 |
| Aalesund | 62 29 | 6 9 | 30·03 | 41 | E.S.E. | 2 |
| Floro | 61 36 | 5 2 | 30·05 | 39 | E.S.E. | 2 |
| Leirdal | 61 6 | 7 27 | 30·31 | 36 | E. | 3 |
| Elverum | 60 53 | 11 33 | 30·59 | 14 | | 0 |
| Fahlun | 60 36 | 15 37 | 30·51 | 15 | | 0 |
| Bergen | 60 24 | 5 20 | 30·15 | 38 | S.S.E. | 2 |
| Helsingfors | 60 10 | 24 57 | 30·89 | | | |
| Kronstadt | 59 59 | 29 47 | 31·00 | −16 | S.E. | 4 |
| St. Petersburg | 59 56 | 30 16 | 31·09 | −16 | | 0 |
| Christiania | 59 55 | 10 45 | 30·58 | 22 | | 0 |
| Reval | 59 26 | 24 45 | 30·98 | −12 | S. | 4 |
| Carlstad | 59 23 | 13 30 | 30·67 | 21 | S. | 1 |
| Baltisch Port | 59 21 | 24 3 | 31·00 | −11 | S. | 4 |
| Stockholm | 59 20 | 18 5 | 30·71 | 21 | S. | 1 |
| Skudesnes | 59 9 | 5 16 | 30·24 | 33 | E.S.E. | 4 |
| Sandosund | 59 5 | 10 27 | 30·57 | 23 | E. | 2 |
| Dorpat | 58 23 | 26 43 | 31·07 | −22 | S.E. | 2 |
| Mandal | 58 2 | 7 27 | 30·47 | 27 | E.S.E. | 4 |
| Westervik | 57 46 | 16 38 | 30·61 | 19 | | 0 |
| Göteborg | 57 42 | 11 58 | 30·60 | 18 | S.E. | 1 |
| Wisby | 57 39 | 18 19 | 30·71 | 14 | S.S.E. | 1 |
| Riga | 56 58 | 24 6 | 30·92 | −15 | S.E. | 4 |
| Kalmar | 56 40 | 16 21 | 30·74 | 16 | S.E. | 1½ |
| Halmstad | 56 40 | 12 51 | 30·51 | 19 | S.E. | 3 |
| Mitau | 56 39 | 23 44 | 30·93 | −15 | | 0 |
| Lund | 55 56 | 13 8 | 30·50 | 15 | E.S.E. | 2 |
| Moskau | 55 46 | 37 40 | 30·98 | −15 | N.E. | 2 |
| Wilna | 54 41 | 25 18 | 30·77 | −17 | E. | 2 |
| Leeuwarden | 53 12 | 5 50 | 30·10 | 27 | E.S.E. | 2 |
| Groningue | 53 12 | 6 34 | 30·11 | 26 | S.E. | 2 |
| Helder | 52 58 | 4 45 | 30·02 | 33 | E.S.E. | 2 |
| Warschau | 52 13 | 21 1 | 30·71 | −13 | E. | 4 |
| Utrecht | 52 5 | 5 7 | 30·10 | 34 | S.E. | 1 |
| Oxford | 51 45 | 1 16w. | 29·94 | 42 | | |
| Greenwich | 51 29 | 0 0 | 29·85 | 43 | S. | 3 |
| Vlissingen | 51 25 | 3 35 | 30·02 | 42 | S. | 3 |
| Leipzig | 51 20 | 12 24 | 30·32 | | E. | 1 |
| Dunkerque | 51 2 | 2 22 | 30·06 | 41 | S.S.E. | 1 |
| Maastricht | 50 51 | 5 41 | 29·85 | 41 | N.E. | 2 |
| Brussels | 50 51 | 4 22 | 30·01 | 45 | S.S.W. | 1 |
| Boulogne | 50 44 | 1 30 | 29·90 | 39 | S.S.W. | 1 |
| Kief | 50 26 | 31 31 | 30·65 | −18 | N. | 2 |
| Prag | 50 5 | 14 25 | 30·56 | 13 | | |
| Krakau | 50 4 | 19 57 | 30·66 | −15 | | |
| Lemberg | 49 50 | 24 2 | 30·66 | −19 | | |
| Mezierers | 49 46 | 4 44 | 30·06 | 34 | E. | 1 |
| Cherbourg | 49 38 | 1 36w. | 29·97 | 39 | S. | 2 |

TABLE VI—*Continued.*

| Station. | Lat. | Long. | Barom. sea level. | Therm. Fahr. | Direct. wind. | Force. |
|---|---|---|---|---|---|---|
| Havre | 49° 31′ | 0° 6′ | 29·98 | 41 | S.E. | 1 |
| Paris | 48 50 | 2 20 | 30·02 | 40 | | 0 |
| Versailles | 48 48 | 2 7 | 29·94 | 46 | S.W. | 1 |
| Beaufieel | 48 45 | 0 57w. | 29·97 | 43 | S.W. | 1 |
| Lugan | 48 35 | 39 20 | 30·67 | — 3 | E. | 6 |
| Strasbourg | 48 35 | 7 45 | 30·20 | 22 | | 0 |
| Wien | 48 12 | 16 22 | 30·38 | 10 | E. | 1 |
| Lorient | 47 44 | 3 21w. | 29·93 | 40 | W.N.W. | 1 |
| Debreczin | 47 32 | 21 41 | 30·36 | — 3 | | |
| Besançon | 47 14 | 6 1 | 30·12 | 32 | N.E. | 1 |
| Kischinef | 47 1 | 28 50 | 30·46 | —11 | N. | 2 |
| Nikolajef | 46 58 | 31 58 | 30·41 | — 9 | N. | 2 |
| Berne | 46 57 | 7 26 | 30·11 | 26 | | |
| Napoleon Vendee | 46 40 | 1 38w. | 30·09 | 41 | N.E. | 1 |
| Odessa | 46 29 | 30 44 | 30·58 | —11 | N.E. | 4 |
| Scegedin | 46 15 | 20 8 | 30·42 | 11 | | |
| Dniestrowski | 46 5 | 30 29 | 30·66 | — 5 | N.E. | 8 |
| Rochefort | 45 56 | 0 58w. | 29·94 | 44 | | |
| Limoges | 45 51 | 1 14 | 29·99 | 41 | S.W. | 1 |
| Agram | 45 49 | 15 55 | 30·20 | 22 | | |
| Lyon | 45 45 | 4 49 | 30·18 | 41 | S. | 1 |
| Triest | 45 39 | 13 46 | 30·14 | 38 | | |
| Turin | 45 11 | 7 41 | 30·18 | 34 | | 1 |
| Pola | 44 52 | 13 50 | 30·12 | 44 | | |
| Bordeaux | 44 50 | 0 32w. | 30·00 | 43 | | |
| Sewastopol | 44 37 | 33 33 | 30·26 | 14 | N.E. | 4 |
| Montauban | 44 1 | 1 20 | 30·10 | 41 | S.W. | 1 |
| Florence | 43 46 | 11 14 | 30·16 | | | |
| Ancona | 43 36 | 13 33 | 30·17 | 39 | | |
| Bayonne | 43 30 | 1 30w. | 29·98 | 43 | | |
| Cette | 43 23 | 3 40 | 30·07 | 43 | N.E. | 1 |
| Marseille | 43 18 | 5 23 | 30·04 | 43 | S.E. | 1 |
| Bilbao | 43 15 | 2 59w. | 30·07 | | | |
| Lesina | 43 11 | 16 27 | 30·09 | 44 | | |
| Civita Vecchia | 42 4 | 11 45 | 30·02 | 50 | S.E. | 2 |
| Rome | 41 54 | 12 29 | 30·09 | 47 | | 0 |
| Barcelona | 41 23 | 2 11 | 30·03 | | | |
| Durazzo | 41 19 | 19 28 | 30·15 | 36 | | |
| Oporto | 41 9 | 8 37w. | 30·33 | | | |
| Palma | 39 40 | 2 40 | 29·96 | | | |
| Palermo | 38 7 | 13 22 | 29·99 | | | |
| Tarifa | 36 3 | 5 35w. | 30·28 | | | |

The following table shows the state of the barometer, etc., at several additional stations, most of them being situated in the northern half of the low area.

On the accompanying map, the isobars are drawn at intervals of two-tenths of an inch from 31 inches to 27·4 inches. The direction of the winds is also shown for a considerable number of stations, and its force is indicated by the number of feathers on the arrows (scale 1–6).

We cannot well avoid the conclusion that an area of high pressure so remarkable as 31 inches situated so near to an area

| Station. | Latitude. | Longitude. | Barometer. | Thermometer. Fahr. | Wind. | |
|---|---|---|---|---|---|---|
| | | | | | Direction. | Force. |
| Sabine Island | 74° 32′ | 18° 49′ | 29·61 | −2 | | |
| Jacobshavn | 69 13 | 50 55 | 29·54 | 3 | NE | |
| Stykkisholm | 65 5 | 22 46 | 28·92 | 34 | East. | 7 |
| Harrison Pt. | 64 55 | 67 15 | 29·98 | −30 | WNW | 2 |
| Reykiavig | 64 20 | 22 0 | 28·62 | 39 | NE | |
| Godthaab | 64 11 | 51 46 | 29·29 | 16 | NE | 4 |
| Iviktut | 61 12 | 48 11 | 29·19 | 25 | West. | 2 |
| North Unst | 60 45 | 0 55 | 29·74 | | | |
| St. John's | 47 30 | 53 15 | 30·10 | 39 | SW | 1 |
| Terceira | 38 39 | 27 15 | 30·01 | | SW | |

of low pressure still more remarkable is not the result of accident. From Jan. 29th to Feb. 8th a low area of unusual magnitude prevailed over the Atlantic Ocean, and during all this time an area of high pressure prevailed over Europe with unusual persistence and at length attained an extraordinary magnitude. At St. Petersburgh the barometer rose steadily from 764$^{mm}$ on the 29th of January to 790$^{mm}$ on the 6th of February. We know that within a low area the air ascends, and that within a high area it descends. It seems natural to conclude that the air which ascended from the low area prevailing over the Atlantic Ocean descended in the high area which prevailed over Northeastern Europe. According to this view, the high area would be (at least in part) the *effect* of the low area. The outward flow of air from the high area must supply the inward flow of some low area. This high area in Northeastern Europe, Feb. 5th seems to have supplied in some degree the air for the low area over the Atlantic, but it could not have been the principal supply, for the movement of the air from the high area was sluggish. But on the 6th of February the outward movement from the west side of the high area became stronger, and this movement became still more decided on the 7th and 8th, at which time the low center had reached the coast of Ireland. Thus high areas appear to be built up by low areas, and high areas become *feeders* to low areas.

Table I, page 444 will enable us to construct the curve which represents the path of a particle of air in its progress towards the center of an area of low pressure. Column 6th shows the inclination of this curve to the isobars at a series of points midway between the isobars shown in column 1st. Plate III exhibits these curves for an area of low pressure and also for an area of high pressure. There are certain important particulars

in which this figure does not correspond to nature. The isobars about a low center in a great storm are never exact circles; and when an area of high pressure meets an area of low pressure the isobar of 760$^{mm}$ tends to assume the form of a straight line.

We see from this figure that the path of a particle of air in its progress towards a low center is *not* a logarithmic spiral. The angle which this curve forms with the isobars is not constant, but diminishes in approaching the center of the low area. This change in the angle of inclination is not simply an occasional occurrence, but is an *invariable* characteristic of great and violent storms.

In my 2d and 8th meteorological papers I gave a table showing the mean distance between the isobars in the neighborhood of a storm's center (the isobars being drawn at intervals of one-tenth inch) for all velocities of the wind from zero to 50 miles per hour, according to the United States Signal Service maps. If for these velocities we compute the corresponding gradients we shall find that the results do not accord well with the preceding formulas. According to these formulas the gradient should be zero when the velocity of the wind is zero; and for a given locality the gradient should increase nearly in the same ratio as the velocity of the wind. But according to the tables above mentioned, when the velocity of the wind is zero, the gradient amounts to 0·075 inch for a distance of 100 miles, and for a velocity of 50 miles per hour the gradient is only three times as great as for a velocity of zero. These anomalies result partly from the mode in which the tables were constructed. A sheet of paper was ruled with 40 vertical columns and at the head of the columns were placed the numbers 0, 1, 2, 3, etc., to denote the velocity of the wind. For each station within an area of low barometer the distance between the two adjacent isobars was measured, and the distance recorded under the corresponding velocity found at the top of one of the vertical columns. Now it frequently happens that the velocity of the wind at some station is zero; but according to the Signal Service maps the gradient is never zero. Indeed it frequently happens that at some station the velocity of the wind is zero when the gradient is very steep. The following examples taken from the volume of observations for September, 1877, will show the truth of this statement. In each of these cases the velocity of the wind was reported to be zero.

It is evident that in these cases the gradients were not the result of a calm, but they resulted from a strong general movement of the atmosphere, and the calms reported at certain stations were exceptions to the general movement of the atmosphere and resulted from local causes. If we combine these steep gradients with the small gradients which usually accom-

pany feeble winds we obtain an average gradient which is considerably too large.

| Date. | Station. | Barometer. | Gradient to 1°. |
|---|---|---|---|
| 1877, Sept. 3.3 | St. Paul, Minn. | 29·80 | 0·070 inch. |
| 4.1 | St. Paul, Minn. | ·80 | ·097 |
| 10.1 | Knoxville, Tenn. | ·89 | ·065 |
| 12.3 | Duluth, Minn. | ·90 | ·075 |
| 13.3 | Duluth, Minn. | ·70 | ·097 |
| 21.1 | Albany, N. Y. | ·90 | ·062 |
| 22.1 | Duluth, Minn. | ·78 | ·080 |
| 28.3 | Ft. Sully, Dak. | ·42 | ·092 |

On the other hand we sometimes find stations where the wind is very high and the gradient is small. If we examine the 250 cases of winds amounting to 40 miles per hour reported in my 8th paper we find that in several of them the gradient was very small. This will be seen from the following list where column sixth shows the gradient to one degree expressed in decimals of an inch.

| Date. | Station. | Barom. | Wind. | | Gradient to 1°. |
|---|---|---|---|---|---|
| | | | Direct'n. | Veloc'y. | |
| 1872, Oct. 7.1 | Indianola, Tex. | 29·99 | N | 43 | 0·053 inch. |
| 1873, Jan. 8.2 | Key West, Fla. | 30·03 | N | 40 | ·059 |
| 16.2 | Punta Rassa, Fla. | 29·93 | NW | 41 | ·049 |
| Feb. 22.3 | Indianola, Tex. | 30·17 | N | 48 | ·049 |
| Nov. 5.2 | Galveston, Tex. | 30·05 | N | 40 | ·052 |
| 12.1 | Galveston, Tex. | 30·21 | N | 40 | ·052 |
| Dec. 9.1 | Key West, Fla. | 30·18 | NE | 40 | ·052 |
| 12.3 | Indianola, Tex. | 30·22 | N | 40 | ·052 |
| 1874, Jan. 4.1 | Indianola, Tex. | 30·12 | N | 48 | ·063 |
| 4.2 | Indianola, Tex. | 30·25 | N | 40 | ·063 |
| Apr. 17.1 | Indianola, Tex. | 30·05 | N | 40 | ·063 |

It will be noticed that all these stations are on the coast of the Gulf of Mexico; that in each case the wind was northerly, and all (with perhaps one exception) occurred during the prevalence of what is popularly known as a *norther*. Now when we combine these cases of small gradients with the cases of steep gradients which usually attend a wind of forty miles per hour, the average value of the gradient is very much reduced. Thus we find that by combining in a common average the observations from all portions of the United States in the manner adopted in my second paper, we obtain an average gradient which is too great for the small velocities of the wind, and too small for the high velocities. For a velocity of the wind amounting to about twenty-two miles per hour, the gradients

found in my second paper, accord very well with those shown in table II on page 447 of the present paper. These results then, when carefully examined, do not appear to be inconsistent with the results of the present article; but in order to test in a satisfactory manner the formulæ here given, the observations should be classified so as to show, not merely the velocity of the wind corresponding to a given gradient, but also the barometric pressure, the temperature of the air, the inclination of the wind to the isobars, and the distance of each station from the center of low pressure. It also seems undesirable to combine in a common average observations from stations which differ greatly in their local peculiarities.

Several European meteorologists have determined the barometric gradient corresponding to different velocities of the wind, by comparing observations at a single station with observations at neighboring stations. Messrs. Whipple and Baker have determined the barometric gradients at Kew by comparing the Kew observations for five years with observations made at other stations distant about 100 miles. The gradients thus determined accord well with the results of the present paper, as will be seen from the following table, in which column first shows the velocity of the wind expressed in meters per second, and column second shows the corresponding gradient expressed in millimeters for a distance of one degree. The inclination of the wind to the isobars was found to be nearly the same for all velocities of the wind and is taken equal to 38°. The distance of Kew from the centers of low pressure is not given, but since 60 per cent of all the observations were employed, it is pre-

BAROMETRIC GRADIENTS FOR KEW, ENGLAND.

| Velocity of Wind. | Gradient for 1°. | G′ | G″ | G-G″ |
|---|---|---|---|---|
| 11·40 | 3·048 | 1·772 | 2·914 | +0·134 |
| 9·92 | 2·743 | 1·546 | 2·535 | +·208 |
| 9·83 | 2·540 | 1·532 | 2·512 | +·028 |
| 8·54 | 2·235 | 1·331 | 2·183 | +·052 |
| 7·38 | 2·032 | 1·150 | 1·886 | +·146 |
| 6·70 | 1·727 | 1·044 | 1·712 | +·015 |
| 5·63 | 1·524 | ·877 | 1·439 | +·085 |
| 5·19 | 1·219 | ·809 | 1·326 | −·107 |
| 4·11 | 1·016 | ·641' | 1·050 | −·034 |
| 3·35 | ·711 | ·522 | ·856 | −·145 |
| 3·13 | ·508 | ·488 | ·800 | −·292 |
| 2·23 | ·203 | ·347 | ·570 | −·367 |

sumed that in about half of the cases the pressure was above the mean, and that in most of the remaining cases the radius of the isobars was large. We are therefore obliged to neglect

the direct effect of centrifugal force, and it is presumed that the error resulting from this omission is not very great. Since the pressure and temperature of the air corresponding to the different wind velocities is not given, we are obliged to neglect the influence of these terms. The latitude of Kew is 51° 28′. Under these conditions Ferrel's formula becomes

$$G = 0{\cdot}1558s,$$

from which are deduced the values in column third. These values (with the exception of the last) are all less than the observed gradients shown in column second. If we multiply each of the numbers in column third by 1·64, we obtain the numbers in column fourth. The differences between the numbers in column second and those in column fourth, are shown in column fifth. It will be seen that for velocities of the wind greater than twelve miles per hour, the corresponding gradients in column fourth are less than the observed gradients; but for velocities less than twelve miles per hour, the gradients in column fourth are greater than the observed gradients. The differences shown in column fifth may in part be ascribed to the factors of the formula which we have been obliged to neglect.

These results confirm the conclusions previously stated in this paper, and appear to demonstrate that the principles of Ferrel's formula are correct, except that the effect of friction is considerably greater than he has supposed.

The preceding paper was written and presented to the National Academy of Sciences last April, but its publication has been delayed by sickness.

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ART. LI.—*A Brief Study of Vesta;* by M. W. HARRINGTON.

IT has long been suspected that several of the asteroids have variable light, but so far as I know they have not been studied with reference to this point except by Ferguson over thirty years ago. One of the tasks which I have desired to undertake has been that of testing more accurately the variability of the asteroids, and, last spring, while studying the character of observations made with the wedge-photometer, I embraced the opportunity of taking a series of observations on Vesta while near her stationary point. She was at this time not far from Argelander's stars, Durchmusterung, zone 22°, numbers 2163 and 2164, of which the former was fainter, the latter brighter than Vesta. My observations were made by taking on 2164, Vesta and 2163 each, five observations of extinction in as rapid succession as possible. The mean of the five extinctions of

each body was taken and is entered as the datum in the following table. The variations in these numbers for the stars are probably due to sources of photometric error, but I took especial pains to have these errors the same for all three of the bodies observed so that they would be eliminated in a comparison of the three. I cannot hope that I have entirely succeeded, but it was my special study to do so.

| Sidereal time of observation. | 2164. | Vesta. | 2163. | Magn. of Vesta. |
|---|---|---|---|---|
| April, 1883. d. h. m. | s. | s. | s. | m. |
| 9 XII 17 | 50·8 | 39·6 | 31·6 | 7·34 |
| 13 XII 31 | 46·4 | 37·5 | 25·5 | 6·79 |
| XIV 57 | 38·3 | 32·4 | 19·2 | 6·38 |
| XV 24 | 36·2 | 29·0 | 16·7 | 6·59 |
| 15 XII 38 | 44·7 | 36·0 | 24·0 | 6·77 |
| XIII 19 | 46·8 | 38·7 | 25·8 | 6·65 |
| XV 43 | 41·5 | 33·9 | 21·7 | 6·55 |
| 16 X 3 | 47·8 | 38·1 | 27·2 | 6·95 |
| XI 55 | 46·4 | 37·1 | 25·6 | 7·07 |
| XII 26 | 47·2 | 38·8 | 25·8 | 6·66 |
| XII 55 | 46·9 | 36·6 | 25·7 | 7·00 |
| XIII 24 | 44·3 | 35·2 | 24·5 | 6·91 |
| XIII 49 | 44·8 | 35·1 | 24·9 | 7·01 |
| XIV 18 | 43·4 | 32·6 | 22·5 | 7·01 |
| 17 IX 39 | 44·6 | 36·0 | 23·5 | 6·76 |
| X 11 | 49·5 | 39·9 | 28·2 | 6·71 |
| X 40 | 48·7 | 40·1 | 27·5 | 6·72 |
| XI 5 | 47·5 | 37·7 | 27·1 | 6·98 |
| XI 29 | 46·7 | 37·9 | 25·4 | 6·75 |
| XI 50 | 46·8 | 37·9 | 25·4 | 6·76 |
| XII 11 | 45·2 | 36·1 | 24·5 | 5·84 |
| XII 31 | 45·5 | 37·0 | 26·2 | 6·85 |
| 19 XII 41 | 45·3 | 35·8 | 24·8 | 6·92 |
| 23 XII 8 | 47·6 | 36·5 | 26·6 | 7·12 |
| XII 58 | 48·3 | 36·9 | 26·7 | 7·15 |
| 26 XI 55 | 50·7 | 39·2 | 30·2 | 7·25 |
| 28 XII 27 | 52·0 | 40·7 | 31·2 | 7·20 |

In the first column are given the Ann Arbor sidereal times of the middle of the set of observations on the three bodies. In the second, third and fourth are the times taken by the stars from entrance on the wedge to extinction. Argelander gives as the magnitude of the first comparison star 5·3 and of the second 8·8. If we adopt these magnitudes and assume that the stars do not vary during the course of the observations, we can easily determine the magnitude of the asteroid at the time of observation. As the magnitude of the star and the time of extinction in the wedge change as the logarithm of the light, they can be compared directly. The fifth column was formed by the proportion,

$$m_1 - m_2 : m_1 - m_3 :: e_1 - e_2 : e_1 - e_3,$$

where $m_1$, $m_2$, $m_3$ and $e_1$, $e_2$, $e_3$ are the magnitudes and times of extinction of 2164, 2163 and Vesta respectively. The change in brightness of Vesta due to change in distance from the earth would not be appreciable from day to day. Indeed, for the whole interval during which observations were taken it would be but about 0·25 of a magnitude. It may therefore be neglected for the present purpose.

Can we conclude from the observations that Vesta is variable? I think that we can. A test of the accuracy of the observation is obtained by comparing the difference in time of extinction of the two stars. It is fairly uniform and generally decreases at the later hours as the stars approached the horizon—as it should do. The errors arising from this source would be large if we attempted to obtain Vesta's light by comparison with one star, but they are eliminated by comparing her with two between which she falls in brightness.

As to the first magnitude obtained (which is the lowest of all) the record gives no evidence of its being less trustworthy than the others. On the contrary, Vesta was at that time in the best position for comparison, and I had already had some practice in comparing her with a single star. Her faintness at that time was so great as to strike my attention before putting the wedge on my instrument. But leaving that out, the observations show a maximum at about $\text{XV}^{\text{h}}$ on the 13th, one approaching on the 15th at the close of the observations, a curious variation on the 16th, and a minimum about $\text{XI}^{\text{h}}$ on the 18th. In general, Vesta is brighter on the 17th than on the 16th, while as she was leaving opposition she should have been slightly fainter if her light is not variable.

The series of observations on the 16th and 17th were taken with the hopes of finding some periodicity which might lead to some determination of Vesta's rotation on her axis, but without entire success.

A few remarks as to Vesta's albedo may not be out of place. Several attempts have been made to measure her diameter, and though the measures are inharmonious, they agree among themselves better than do the different measures of other asteroids—as Ceres and Pallas. The mean of the measures of Schröter, Mädler, Secchi, Tacchini and Millosevich, reduced to Vesta's mean distance from the Sun, as given by Houzeau (Vade-mecum, p. 637), I find to be 0″·49. Employing this, using Zöllner's formula for computing albedo (Phot. Untersuchungen, page 159), and giving Vesta a purely geometrical phase, I find her albedo to be about 0·1 or much like that of the Moon and Mercury. This suggests that she has something of the physical condition of the Moon, which is also suggested by her size. We may note, however, that if her angular diame-

ter at mean distance from the Sun is 0″.49, her diameter in miles must be not inconsiderable. In fact, a simple calculation shows that with this angular diameter the diameter is about 520 miles, making her more than four times as large as Argelander and others made her by assuming her to have the same albedo as Saturn. Indeed, we may doubt if this assumption is justified on general principles. The albedo of Saturn is peculiar and is approached only by that of Neptune. The asteroids are much more likely to be like their nearest neighbors, Jupiter or Mars. Considering their small size, however, and remembering how important a part the size plays in the process of cooling, causing presence or absence of clouds, water, etc., we may consider it more probable that these bodies resemble those other small bodies, the Moon and Mercury.

As a result of my observations and of the other considerations just mentioned, we may, I think, conclude that the present state of knowledge renders the following conclusions probable:

1. Vesta is a body upwards of 500 miles in diameter.
2. She is like the moon in her albedo and therefore probably like her in lacking an appreciable atmosphere and water.
3. To account for the irregularities of her light, we may presume that she has a very rough surface and rotates on her axis. The time of rotation can not be guessed at, but the rapidity of the changes in her light indicates that it is short.
4. What is true of Vesta is likely to be true, *mutatis mutandis*, of the other asteroids.

Subsequent observation may modify these conclusions materially, or may prove that the asteroids are even more dissimilar in their physical characters than they are in the elements of their orbits. Until we are better informed, however, the above may be of interest as the only indications we have of the physical condition of this large family of planetary bodies.

Ann Arbor, Michigan, Oct. 10, 1883.

ART. LII.—*On a New Form of Selenium Cell, and some Electrical Discoveries made by its use;* by CHARLES E. FRITTS, of New York City.

IN the following pages I give, in a condensed form, the chief results brought out in a paper presented by me to the American Association for the Advancement of Science, at the meeting at Minneapolis in August last.

The new form of selenium cell which I have devised has the following features:

1st. Its resistance can readily be made as low as desired. Some cells have been made having a resistance as low as nine ohms. But I have generally used those measuring between 500 and 5,000 ohms.

2d. The light is caused to strike the cell in the same plane or general direction as the current.

3d. They are made in any form to be easily handled and used, and are practical working apparatus, both for scientific researches and technical uses. The cost, when completed, is ordinarily about $100 each.

4th. They are far more sensitive to light than any before known. One cell was cited which had fifteen times as high resistance in dark as in ordinary diffused daylight in a room.

5th. They exhibit other properties in an equally high degree, as specified in the paper.

The process of purifying the selenium was first explained, resulting in the separation of the product into a number of portions, having very perceptible differences of behavior, both in melting and annealing. One portion would become soft while held in the hand, and could not be made to anneal properly at any temperature, while another portion would anneal and become hard at a low heat and could not be melted without previously going through the annealing process. Yet both are supposed to be absolutely chemically pure selenium. The conclusion is that commercial selenium is a mixture, consisting of several distinct portions, which must either be different allotropic forms of the element, or new elements, similar in appearance, chemical reactions and physical properties, yet distinctly different in many respects.

I have found the "vitreous" form, or quickly cooled selenium, to have many different colors even from the same small piece of the chemically pure substance,—varying from very dark brown, almost black, through shades of brown, blue, purple, and violet, to a beautiful rose-red hue. These are distinct from the magnificent ruby-red color which vitreous selenium shows when a very thin sheet of it is viewed by transmitted light. The granular or crystalline form also varies,—being

sometimes of a very light lead color, at others a very dark gray, a violet, a purple, a dull gold color, and occasionally a blue. These colors are caused by heat.

I have tried many different metals and substances as bases for selenium cells, including iron, brass, copper, zinc, tin, lead, platinum, silver, gold, nickel, german silver, aluminum, bismuth, carbon, wood, mica and glass. My preference is for brass, zinc, or iron or copper thinly coated with tin.

Descriptions were given of cells of the forms previously known, especially of the "strip" cell, made for comparative tests with my own form. The sensitiveness to light of cells heretofore made has usually been quite low—one whose resistance would fall to one-half, or 50 per cent change, by exposure to sunlight, was uncommonly good. Probably the average of results would not exceed 10 per cent of change from dark to light. The most sensitive selenium cell on record up to this time was that of Dr. Werner Siemens, which was stated to have a resistance in dark 14·8 times as great as in sunlight. His measurements of this cell are given in table A, annexed hereto. Table B gives several measurements of a cell of my form, and the paper also gave the measurements of a number of other cells of my construction to show their extreme sensitiveness to light.* A lot of my cells, five in number, presented

* I have just been measuring a new series of selenium cells of my form and find several of them far more sensitive to light than any of those mentioned in my paper before the American Association for the Advancement of Science. Their sensitiveness is so extraordinary that I hasten to put them on record. Brass cell No. 24, measured Sept. 4th, showed twenty-five times the conductivity in sunlight that it had in the dark, and brass cell No. 23, showed about thirty times as much,—the exact ratio being 29·63 to 1.

I have measured the same cells again to-day, together with several others, with the results given in the following table. All the cells were measured "gold anode," i. e., with the current entering the cell at the gold electrode, and with 23 elements of Leclanché battery, except Nos. 22, 23 and 8. The time was between 2 and 4 P. M., sky partly cloudy, sunlight good but not first-rate.

TESTS OF SELENIUM CELLS IN LIGHT AND DARK.

| Selenium cell. | Battery. | In dark. | Sunlight. | Ratio. | Date. |
|---|---|---|---|---|---|
| No. 24. | 23 cells. | 32,500 ohms. | 1,300 ohms. | 25· to 1 | Sept. 4, 1883. |
| 24. | 23 " | 48,500 " | 1,100 " | 44·09 to 1 | " 5, " |
| 23. | 10 " | 1,600 " | 54 " | 29·63 to 1 | " 4, " |
| 23. | 10 " | 1,510 " | 50 " | 30·2 to 1 | " 5, " |
| 25. | 23 " | 15,000 " | 750 " | 20· to 1 | " " " |
| 22. | 23 " | 1,530 " | 80 " | 19·12 to 1 | " " " |
| 22. | 10 " | 2,030 " | 130 " | 15·6 to 1 | " " " |
| 6. | 23 " | 5,040 " | 170 " | 29·65 to 1 | " " " |
| 5. | 23 " | 2,400 " | 150 " | 16· to 1 | " " " |
| 10. | 23 " | 60,000 " | 3,600 " | 16·6 to 1 | " " " |
| 17. | 23 " | 1,790 " | 250 " | 7·1 to 1 | " " " |
| 8. | 10 " | 30,000 " | 1,000 " | 30· to 1 | " " " |

I have a number of others not yet tried, which may give even greater results. But is it not astonishing that the electrical resistance of a substance should be

for inspection and trial by the members of the association, was then described.

As the result of some thousands of tests and measurements made by me, I have reached the following new conclusions:

1st. The electrical resistance of a cell changes enormously with different battery powers. There does not appear to be any invariable law governing this change, but each cell seems to have its individual character in this respect. In most cases the resistance becomes lower as the electromotive force of the current increases. One cell cited measured, with one Leclanché element, 14,000 ohms; with 5 elements, 9,900 ohms; with 10 elements, 7,600 ohms; with 23 elements, 4,600 ohms. Another measured, with 23 elements, 3,600 ohms; with 10 elements, 8,000 ohms; with 5 elements, 10,000 ohms. In other cases, but less frequently, the resistance increases as the battery power increases. These changes may be produced in either direction, and as often as desired.*

2d. I have discovered that simply reversing the direction of the current through a cell can make its resistance, in some cases, as much as ten or fifteen times as high as before, even though that increase should amount to millions of ohms. In some instances the change may be even greater, in others not so much, but it is seldom less than twice as much, or as two to one. When the original direction of the current is restored, the resistance also returns, and these effects can be repeated any number of times. The cell is sensitive to light in both cases, but is generally more sensitive when the current enters the selenium at the same surface which the light is acting upon. Instances of such changes were given, and several hypotheses were considered, but none were thought to satisfactorily account for this phenomenon.†

forty-four times as much in the dark as in the sunlight? I also wish to record the fact that I have constructed a selenium cell whose resistance becomes greater in the light and less in the dark,—being, so far as I know, the first instance of that kind ever obtained.—New York, Sept. 5th, 1883.

* In rare instances, the increase of resistance goes on up to a certain battery power, and any further increase of intensity in the current causes a fall in the resistance,—so that a change from that battery power in either direction would produce the same change in the resistance of the cell.

† The foregoing changes are not always in the same direction. That is to say, if the cell has a certain resistance, with the positive pole of the battery connected to a certain electrode of the cell, the resistance will in some cases be increased by connecting the negative pole of the battery to that electrode, and in other cases it will be lowered. Instances have been found where reversing the direction of the current caused no change in the resistance of the cell. And, what is still more singular, I have had two cells which reversed their action while being experimented with,—so that the electrode which offered the highest resistance to the positive current at first, afterwards offered the lower resistance to it, no change having been made in the connections or conditions. The selenium or the cell seemed to have been in some way permanently affected by the action of the current flowing through it. The cause of this change has not been ascertained.

After some speculations as to whether the foregoing actions are properties of selenium, or are produced by the arrangement of selenium in contact with substances so widely separated from it in the electrical scale, I observed that it is at least evident that the peculiar construction of my cells causes these actions to be manifested many times more powerfully by them than by cells of other forms, and then inquired: If it be possible, by such simple means as are employed in my cells, to obtain these results, may not means be found to still further facilitate these manifestations, and so intensify and exaggerate the results,—and even to obtain others yet unthought of, and possibly still more surprising?

3d. Still I have found that the *kind* of battery employed has a great deal to do with the performance of the cell. Take iron cell No. 1 as exemplifying this. This plate is one of the best, having given a change of 83 per cent, has been used in almost every conceivable way, and always proved good. Yet on removing the Leclanché battery which was usually employed, and substituting the small bichromate plunge battery, this plate became apparently sulky, and finally refused to respond at all to the action of light. Changing from 24 cells to 48 cells of battery has but little effect, whether the gold is the anode or cathode. It is still absolutely insensitive to light.

But on changing the battery to 96 cells its resistance jumps from 265 ohms to 11,500 in dark and 5,000 ohms in light, resuming its old sensitiveness, but apparently under compulsion. On changing to gold cathode, it sinks from 9,000 to 1,750 ohms in dark, then gradually falls to 275 in the dark, and the same in the light. Reversal to gold anode produces no change; it still remains at 275, sinks to 250 dark, and 245 light, and, had another measurement been taken, would doubtless have proved insensitive again. On removing the obnoxious battery for one minute, its resistance springs up to 12,000 ohms in the dark (still gold anode), and 5,000 in light; but gradually falls off again till it shows only 22 per cent change from dark to light. On substituting its old companion, the Leclanché battery, it immediately becomes tractable, and ends up with its original sensitiveness.

Brass cell No. 6, which has given 85 and 88 per cent with Leclanché battery, appeared to be almost worthless with the bichromate cells, showing but little sensitiveness to light,—in one measurement, none at all; and in another, its resistance was actually less in dark than in light,—the figures being 540 and 750 ohms, respectively. But on putting it with the Leclanché, its action changed, it became fairly sensitive to light, and behaved more like its old self.

Brass cell No. 12 also failed with the bichromate battery

and became entirely insensitive to light, showing no change whatever between light and dark.

Brass cell No. 5, when connected with the bichromate battery, refused from the first to show any sensitiveness at all. Its resistance varied continually, from high to low, and up again,—most of the time changing too rapidly to admit of getting any measurement. Changing the battery power from 12 cells to 24, 48, and 96 cells made no difference,—it still refused to respond to light, although reversing the current varied the resistance considerably. When one cell of the Leclanché battery was substituted, it started at 10,100 ohms in dark, and 5,700 ohms in light,—nearly 44 per cent decrease in light.

Doubtless other cells would have shown the same action, had there been time to test them, but these are sufficient to excite one's surprise. As before stated, the bichromate batteries had about the same surface in the liquid as the Leclanché. But even supposing that the current of the former had much greater quantity, intensity, or both, we might expect a change of resistance, *but how could it affect the sensitiveness of selenium to light?* If one kind of battery current destroys its sensitiveness, may we not suppose that another kind of battery might increase its sensitiveness? Although the Leclanché has operated well, some other may operate still better, and by its special fitness for use on selenium cells may intensify their actions, and so bring to light other properties yet unthought of. If so, where is the limit to this method of exalting the properties of selenium?

Is not here a promising field for experiment, in testing the various forms of battery already known, or even devising some new form especially adapted to the needs and peculiarities of selenium cells?

4th. The effect of intermittent currents, and of rapidly alternating currents, is usually very slight and may be disregarded in practical work. But occasionally they produce very surprising changes. Sometimes they reduce the resistance of the selenium cell from many thousands of ohms to almost nothing, and at others they raise it from next to nothing up among the thousands. I have not been able to ascertain any connection between conditions and results, but the effects are certainly remarkable. They are not due to my mode of arranging the parts of the cells, for the same thing occurs with other forms, as for example:

*Experiment* 1. The large "strip" cell measured, in the dark, 1,600 ohms. "Tried intermittent current. Needle erratic,—finally settles at 2,600 ohms." This experiment was with 22 cells of Leclanché. On trying the same experiment on an-

other day, no effect was produced, although the conditions were the same, so far as I could detect.

*Experiment* 2. Double cell No. 1, measured 50,000 ohms. Put on an automatic reversing apparatus (arranged to both break circuit and reverse the current about 300 times per minute), with 22 cells of Leclanché, for three minutes. The cell then measured, under the same conditions as before, only 30,000 ohms. A repetition of this treatment produced no further effect. The change, produced as described, was permanent.

The action of both intermittent and alternating currents upon selenium are worthy of careful study, for under certain conditions they are capable of effecting great changes in its resistance, which might be utilized for practical purposes if those conditions were understood.

5th. Very moderate changes of temperature (say 10° to 50° Fahr.), can sometimes change the resistance of a selenium cell hundreds or even thousands of ohms in a few seconds, and I think that this phenomenon has not been observed by others.

*Experiment* 1. Brass cell No. 6 measured, with gold anode, 23 cells Leclanché, in dark, 3,500 ohms. It was laid with its back upon an iron block warmed to about 100° Fahr., when its resistance fell almost instantly to 2,900 ohms, being a fall of 600 ohms.

*Experiment* 2. The same cell, in the same conditions at another time, measured 3,600 ohms. On putting the hot block under it, it fell to 2,400 ohms; in 30 seconds more it fell to 2,000 ohms; in another 30 seconds, to 1,850 ohms; in two minutes more to 1,600 ohms. By this time the block was but slightly warm.

*Experiment* 3. A cold block (i. e., not heated), was then put under the cell, when it rose at once to 1,920 ohms; in 30 seconds more, to 2,000 ohms; and continued to rise rapidly to 3,010 ohms. On repeating experiments 2 and 3, similar results were again obtained.

It will be observed that these effects were obtained by very small changes of temperature, and the phenomenon is distinct from the ordinary changes in electrical conductivity observed at different temperatures of the conductors, as well as from those produced by very high temperatures. The fact that an increase of temperature, so slight that the heated block barely feels warm to the hand, can change the resistance of a selenium cell many hundreds and even thousands of ohms, almost instantly, and that the withdrawal of so little heat can produce a change in the opposite direction, almost as great and as rapidly, is one of some importance, and has not been published before, so far as I am aware.

This action is more prominent in brass cell No. 6, than in any of the others. The same experiment tried with iron cell No. 1 produced no perceptible change in the resistance.

*Experiment* 4. Double cell No. 1, measuring 33,000 ohms, was laid on warm blocks, and fell instantly 3,000 ohms, then sank rapidly to 22,000 ohms. The same experiment, with double cell No. 2, the mate to No. 1, produced hardly any effect. When the "strip" cell was tried in the same way, it fell slowly, from 1,023 ohms to 955 ohms, in about five minutes. This action therefore appears to be exhibited much more strikingly by cells of my form than by those heretofore made. But why it occurs with some cells and not with others, has not been ascertained. The sensitiveness to light is greater when the cell is cold.

*New form of Selenium.*—Since the foregoing paragraphs were written, I have been trying some experiments long contemplated, and have succeeded in producing *colorless, transparent selenium.*

It is well known that vitreous selenium when very thin becomes translucent and has a beautiful ruby red color. The new form of selenium just discovered by me has no color, but looks like a thin coating of glass, through which the yellow brass base is clearly seen,—even the fine scratches and marks left in the brass from the polishing being as distinct as if the metal were bare. The mode of obtaining it is such as to exclude everything but selenium. The conditions of its production are these: The exposure of the selenium to a prolonged heating, followed by gradual cooling, under pressure. The plates so made have portions which are transparent, situated apparently where the temperature was highest. The remaining portions of the plates are black and red, interspersed with gray. Thus upon one plate may be seen selenium in four different states or colors. As yet, I have been able to make only a partial examination of one of these plates. A small portion of the transparent selenium having, perhaps, one-half square inch of surface, had a resistance of only three ohms. It was found to be affected by light very little, if at all.

As circumstances compel me, much against my will, to devote most of my time to a widely different branch of science, I do not wish to stand in the way of those who would work out these subjects more rapidly, and therefore offer my results for their consideration, and leave further researches to be carried out by any desiring to do so who may have more time and better facilities than myself for theoretical investigations.

TABLE A.

SELENIUM CELL, TESTED BY DR. WERNER SIEMENS.

| Selenium in | Relative Conductivities. | | Resistance in Ohms. |
|---|---|---|---|
| | Deflection. | Ratio. | |
| 1. Dark | 32· | 1· | 10,070,000 |
| 2. Diffused daylight | 110· | 3·4 | 2,930,000 |
| 3. Lamplight | 180· | 5·6 | 1,790,000 |
| 4. Sunlight | 470· | 14·8 | 680,000 |

TABLE B.

SELENIUM CELL, TESTED BY THE AUTHOR.

*Resistance in Ohms.*

| Brass Cell, No. 16. | Test No. 1. | Test No. 2. | Test No. 3. | Test No. 4. | Test No. 5. | Test No. 6. | Test No. 7. |
|---|---|---|---|---|---|---|---|
| Dark | 210· | 210· | 170· | 110· | 210· | 219· | 395· |
| Diffused daylight | 49· | 14· | 53· | 13· | 110· | 90· | 72· |
| Per cent of change | 76·6 | 93·3 | 68·8 | 88·1 | 47·6 | 58·9 | 81·7 |

*Note.*—A paper on "The Action of Light on Selenium," by Professor W. G. Adams, F.R.S., and Mr. R. E. Day, in Proc. Roy. Soc., vol. xxv, p. 113, contains the following statements:

1st. That on reversing the direction of the current through selenium the "resistance was always found to be different from that previously obtained." As I have shown, the resistance is not always different.

2d. "The first current through the selenium, if a strong one, causes a permanent 'set' of the molecules, in consequence of which the passage of the current during the remainder of the experiments is more resisted in that direction than it is when passing in the opposite direction." As already stated, this "set" is sometimes in the direction above laid down, sometimes in the opposite direction, sometimes there is no "set" at all, and in two cases the "set" *changed* and was reversed during the measurements of the cells.

3d. "That with the same piece of selenium at the same temperature, the resistance diminished as the battery power increased." This I have found by experiments with more than one hundred cells to be correct in some cases, while the opposite occurs in other cases, and in still other instances the increase or decrease of resistance stops at a certain battery power, and is then reversed.

It will be seen that their experiments and results were entirely different from mine, except on the three foregoing points. As to them, so far as the above quotations agree with my results, those investigators are entitled to the credit of first discovery.

New York City, Aug. 28, 1883.

ART. LIII.—*The Ischian Earthquake of July* 28, 1883; by C. G. ROCKWOOD, JR., Princeton, N. J.

THE island of Ischia is situated at the western extremity of the bay of Naples, about twenty miles in a direct line from Vesuvius. It forms a part of the Vesuvian volcanic district, being an extension of the Phlegrean Fields with which it corresponds in geological character, as does also the smaller island of Procida lying between Ischia and the mainland. Ischia contains about twenty-six square miles, being eight kilometers or about five miles long east and west, by 4·8 kilometers or about three miles broad. In the center of the island rises Mount Epomeo, 792 meters (2,782 feet) high, an old volcano which during historic times has given no lava from its crater, and for over five hundred years has been nearly silent. The last emission of lava from its flanks occurred in 1302, since which time the volcanic activity has been shown only by hot springs and steam jets, and not infrequent earthquakes, the last notable one having occurred March 4, 1881.

Ischia is noted for its thermal springs, which with the pleasant climate make it a favorite summer resort, and during the season the town of Casamicciola is usually crowded with strangers. Other towns are Ischia on the east coast, Forio on the west and Lacco Ameno on the north.

Before the earthquake, which wrought so much damage in this beautiful island on July 28, 1883, there had been some indications of unusual subterranean activity; some of the hot springs had shown abnormal variations of temperature, but it is doubtful whether to an extent greater than at other times when no earthquake followed;—there had been a number of slight earthquake shocks in different parts of the island,—and the instruments in the seismological observatories at Naples and Rome were in increased motion. It is said also that unmistakable indications of an approaching earthquake were observed in Casamicciola, but were concealed from the public so far as possible, for fear of alarming the summer visitors and so diminishing the gains of the hotel keepers. These indications, beginning from a fortnight before the great earthquake shock, gradually increased in intensity and frequency, and Professor di Rossi of Rome said afterward that if there had been in Ischia any system of observation, by which at the time he could have been informed of all these phenomena, "he would not have hesitated an instant in pointing out the imminent danger of an impending seismic disturbance" menacing Casamicciola. But in the absence of observations in Ischia,

the indications of the instruments on the mainland were interpreted as being fulfilled by the earthquake of July 25th in Calabria. On July 27, however, the unusual noises before noticed at the Solfatara of Albano had increased to an alarming extent, and numerous slight shocks were felt at Vesuvius and vicinity, indicating that the seismic disturbance was not yet over. The violent shock came about 9.25 P. M. on Saturday, July 28, and the greatest damage was done at Casamicciola and vicinity. This town, built on two small hills on the north slope of Epomeo, was entirely destroyed, only one or two houses being left standing. A performance was in progress at the theatre, and when the building collapsed at the shock many were buried in its ruins. Lacco Ameno, on the coast about three miles northwest from Casamicciola, was also mostly destroyed and Forio was greatly damaged. The town of Ischia, on the east coast, was severely shaken, but without suffering very much damage. But the villages of Fontana and Serrara, situated in the interior of the island, and indeed within the old crater, were great sufferers, as was also Barano more to the south. Two large land-slips were caused on the north slope of Epomeo, but no true fissures were found anywhere, and no apparent changes of level.

The first accounts of the killed and wounded were largely exaggerated, the number of killed being stated as 4000, 5000, 8000 and even 13,000. The numbers given by the official commission (Sept. 26) were 1990 killed and 374 wounded. Many perished beneath the ruins of fallen walls, and some were rescued from such a death after having been thus entombed for longer or shorter periods; in one case several persons were taken out of the ruins alive nearly a week after the catastrophe.

The shocks were at first vertical, then undulatory, and lasted about fifteen seconds. The direction of the vibration is variously stated by different observers.

In endeavoring to trace this earthquake to its cause, we must not overlook the connection of this locality with the Vesuvian volcanic district. It will be desirable also to state briefly the location of the hot springs and stufas or steam jets, for which the island is noted. As we follow then the northern coast from east to west, we find thermal springs at Pontano, Fornello and Fontana near Ischia, stufas and thermal springs at Castiglione, stufas at Cacciuto, all near the northern coast, then the abundant hot springs of Gurgitello near Casamicciola, fumaroles at Monte Cito west of Casamicciola, and finally the springs of Paolone on the slopes of Monte Nuovo near Forio. All these lie on a line slightly convex toward the north, and crossing the island from east to west along the northern slopes of Epomeo. They are considered by L. Baldacci to mark an old

line of cleavage, from whence come these manifestations of subterranean activity. He also traces another line similarly marked from north to south; from the hot springs of San Restituta near Lacco Ameno, crossing the first line at Monte Cito, almost under Casamicciola, cutting through the old crater of Epomeo, along the precipitous valley of the Scarrupato, where are found the inland villages of Fontana, Serrara, Meropane and Barano, and meeting the southern coast at the place where are the hot springs of Fondolillo and the stufas of Testaccio. The east-west line above indicated might find its prolongation in the volcanic islet of Procida, and in the elevated point which projects from the mainland opposite it and connects it geologically with the Phlegrean Fields. Now these two lines coincide nearly with the localities, where this earthquake shock was felt, and at their intersection stood Casamicciola which was so completely destroyed.

In view of these facts L. Baldacci states his conclusions thus: "That the residual volcanic activity of the island is manifested along two principal fissures, one a curve with its convexity to the north, from the baths of Ischia to Forio, the other directed approximately north-northwest and south-southeast between Lacco Ameno and the stufas of Testaccio; and that the place where Casamicciola stood is upon the intersection of these two lines and, therefore, at the very focus of seismic activity and that it always has been and always will be the locality most liable to be devastated by earthquakes."

But other facts also point in the same direction. H. J. Johnston-Lavis has drawn the isoseismal lines about the focus and finds them to be elongated ellipses having their major axes east and west, the lines lying much closer together in their northern portion than in the southern. The minor axes of these isoseismal ellipses is *nearly in the line* of Baldacci's north and south fissure. Johnston-Lavis states his conclusion thus: "From a careful examination of observed azimuths and angles of emergence, all point to a plate-shaped focus whose strike extends in a line from Fontana just west of Menella to near the beach at Lacco. The plane of this fissure is probably roughly perpendicular to the surface, but may dip slightly toward the east, as the isoseismals are slightly nearer on the eastern side of the seismic vertical, which, as a necessity, is not represented by a point, but by a line on the surface. The rupturing of this plate-like fissure was apparently greatest at a point nearly midway between its extremities."

To conclude, then, it seems to be pretty clearly made out as probable, that this earthquake shock had its origin and cause in a rupture taking place along an old volcanic fissure, directed roughly north and south, and extending radially in or under

the northern slope of Epomeo; and that the cause of the increased tension resulting in this rupture is to be referred to the residual volcanic activity which this island shares with the adjacent mainland rather than to any merely local subsidence.

Palmieri has advanced the opinion that the local violence of the shocks might be due to the collapse of subterranean cavities not very far below the surface. But it would seem likely that any subsidence of sufficient magnitude to cause the surface destruction, seen at Casamicciola, and to give rise to vibrations felt by instruments so far away as Rome, must have been attended also by surface fissures and changes of level, none of which were observed.

After the disastrous earthquake of July 28, slight shocks continued to occur, at first daily, and afterward at longer intervals, even as late as September 23d.

In preparing this notice free use has been made of the correspondence published in various journals and newspapers both of America and Europe, but the writer is especially indebted to articles contributed by Ch. Vélain to *La Nature* (August 18, 1883); by H. J. Johnston-Lavis to *Nature* (Sept. 6, 1883), and by L. Baldacci to the Boll. R. Com. Geol. (translated in Science, Sept. 21, 1883.)

## SCIENTIFIC INTELLIGENCE.

### I. Chemistry and Physics.

1. *A method of isolating heat radiation from the luminous and actinic radiations.*—In a note communicated to the French Academy, M. F. Van Assche describes his process, which consists of depositing a thin film of distilled and melted selenium upon a plate of glass which he designates by the term *porte-object.* The selenium is immediately covered with a thin plate of glass called the *couvre-object.* The selenium is pressed into a thin plate by means of heat and by pressure, taking care not to boil the selenium, to avoid bubbles and striæ. The light transmitted through this thin plate of selenium is mono-chromatic, and of a reddish tint. Seen through the spectroscope, the light is comprised between the A line and the C line. The double line B and the numerous small lines *a* are readily seen, but the D lines can not be observed. There is, so to speak, a transformation of the luminous radiation of the sodium lines. A great band of absorption lies between A and C. It appears that the luminous, heat, and actinic radiations are interrupted by the selenium when the source of these radiations has a temperature between 720° and 2000° and perhaps transformed into electrical energy. The radiations due to comparatively low temperatures, 525° to 720°, alone can penetrate the selenium. A thin plate of selenium heated to 250° can

convert all the radiations into obscure radiations. One can utilize this specific property of selenium in analyzing the heat rays; also in a photographic chamber; in the use of the opthalmoscope, and generally in the analysis of the radiation of the sun and the stars. —*Comptes Rendus*, No. xvi, 15 October, 1883, pp. 838–840.

J. T.

2. *Telluric Oxygen lines.*—M. EGOROFF has shown that the lines A and B of the solar spectrum are due to the oxygen in the earth's atmosphere. He employed a tube twenty meters long closed at both ends by glass plates; filled this tube with dry oxygen under a pressure of fifteen atmospheres; observed the light from an oxy-hydrogen flame and found that the lines due to the absorption of the oxygen were identical with A and B in the solar spectrum. The theory that these lines are due to a cosmical hydrocarbon gas diffused through space is controverted by the experiments of M. Egoroff, who found that several kinds of hydrocarbon gas gave no bands or lines of the character of A and B.—*Comptes Rendus*, 27 August, 1883, pp. 555–557. J. T.

3. *A new Capillary Electrometer.*—M. A. CHEVET has devised a modification of Lippmann's Electrometer which can be readily constructed and which will show a difference of potential from $\frac{1}{1000}$ to $\frac{1}{10000}$ of a volt. Two flasks with lateral orifices on the same horizontal line are connected through these orifices by the tube of a thermometer open at both ends. The bulb end enters the flask A which is filled with mercury. The capillary end enters the flask B. This latter flask is filled partly with mercury and partly with water acidulated with $\frac{1}{10}$ part of sulphuric acid. The capillary end of the thermometer enters the acidulated water. A platinum wire, P, insulated by a vitreous covering so as not to be in contact with the acidulated water, is in contact with the mercury of flask B. Another platinum wire, N, is in contact with the mercury of the flask A. By means of a commutator a difference of potential can be intercalated between the ends of P and N. The heights of the mercury and water in the flasks A and B are such that P and N being connected by a metal wire, the surfaces of separation of the two liquids is in the region of the capillary portion of the larger end of the thermometer tube. Let $\alpha$ be the angle of the cone which is tangent to the surface of the tube at the point where the meniscus is formed; $\alpha$ is a very small angle. Let $a$ be the capillary depression and $r$ the radius of the tube. Then $a = \frac{M}{r}$ where M is dependent upon the difference of potential intercalated between P and N. The movement of the meniscus is observed with an eye-piece. By means of a manometer attachment differences of potential can be compared by the method of Lippmann.—*Comptes Rendus* pp. 669–672, 17 September, 1883.

J. T.

4. *Hall's Phenomenon.*—M. Aug. Righi modifies the form of thin plate used by Dr. Hall; instead of a cruciform plate it has any shape whatsoever and carries three electrodes. The current

enters or departs by one of the electrodes, and departs or enters by the two others. The two partial currents circulate in opposite directions through the two circuits of each of the bobbins of a Wiedemann galvanometer. A suitable resistance having been introduced the needle of the galvanometer can be maintained at zero. When one places this arrangement of partial currents between the poles of an electro-magnet the direction of the deviation of the needle shows that the equi-potential lines are turned in the opposite direction from the magnetizing current—for gold and metals which have the same action as gold—and in the same direction as the magnetizing current for iron. By changing the direction of the current in the plate, deviations of the galvanometer needle are obtained which show that the effect cannot be explained by the direct action of magnetism upon the current. M. Righi promises to explain this in a forthcoming memoir. The action in bismuth is very strong, being nearly 5000 times as strong as in gold, with the same strength of current and the same dimensions. A plate of bismuth of $0{\cdot}079^{mm}$ in thickness gives indications five or six times greater than a piece of gold leaf $0{\cdot}000087^{mm}$ in thickness. The effect can be obtained in bismuth with a permanent magnet instead of an electro-magnet, and the author hopes to be able to show Hall's phenomenon by means of the earth's magnetism. J. T.

5. *Note on the Vortex-Atom Theory;* by JAMES CROLL. The vortex-atom theory, viewed from the physical side, is regarded by some as one of extreme simplicity. I must confess, however, that to me it appears to be far otherwise. In fact, I am unable to understand how the theory is to be reconciled with the first law of motion. According to that law no body possessing inertia can deviate from the straight line unless forced to do so. A planet will not move round the sun unless it be constantly acted upon by a force deflecting it from the straight path. A grindstone will not rotate on its axis unless its particles are held together by a force preventing them from flying off in a tangent to the curve in which they are moving. Centrifugal force must always be balanced by centripetal force. My difficulty is to understand what force counterbalances the centrifugal force of the rotating material of the vortex-atom. It has been said that the centrifugal tendency of the rotating material of the vortex-atom is controlled by the exterior incompressible liquid. But it is also stated that this incompressible liquid offers no resistance whatever to the passage of the atom through it. In short, that in so far as the motion of the atom is concerned this liquid is a perfect void. Now, if this liquid can offer no resistance to the passage of the atom as a whole, how then does it manage to offer such enormous resistance to the materials composing the atom so as to continually deflect them from the straight path and compel them to move in a curve? The centrifugal force of these vortex-atoms must be enormous, for on it is assumed to depend the hardness or resistance of matter to pressure. Now the centripetal force which

balances this centrifugal force must be equally enormous. Then if this perfect fluid outside the vortex-atom can exert this enormous force on the revolving material without being itself possessed of motion, then there does not seem to be any necessity for vortex-motion in order to produce resistance.

It has further been advanced, by way of explanation, that this incompressible liquid surrounds the revolving liquid like a pipe, "and that if the liquid in this pipe were to fly out, a temporary void would be formed in it, which is impossible in a liquid that already occupies all space." The incompressibility of the surrounding fluid surely cannot be a reason why portions of the revolving material do not fly out, for if incompressibility could prevent portions of the revolving atom from flying away, it would equally prevent the whole atom from doing so, but according to hypothesis this incompressible fluid offers no resistance to the motion of the atom. When the atom moves it is assumed that the fluid in front is displaced; but then this simply makes room for an equal quantity behind, and thus no void is formed. The very same thing ought to take place, though only a portion of the atom were to fly off. The various portions of this revolving material are not supposed to be held together by any cohesive force like those of the grindstone. What then prevents the revolving material from being dissipated by the centrifugal force of rotation? In short, how is the existence of the atom possible under the physical conditions assumed in the theory?—*Knowledge, Aug.* 17; *proof communicated by the author.*

## II. Geology and Natural History.

1. *Geology of the Comstock Lode and the Washoe District;* by George F. Becker, U. S. Geological Survey, Clarence King, Director. 422 pp. 4to, with many plates, and an Atlas of 21 sheets in double folio, containing maps and underground sections of the region.—Mr. Becker's Report, after some introductory remarks, treats exclusively of the geology of the Comstock lode, and is a work of great value. Some of its facts and results are here briefly presented.

Tables prepared with care by Mr. Eliot show that this so-called *Silver* lode has afforded up to July, 1880, (or in 21 years) about 43 per cent of gold to 57 of silver, the whole value of the two about $306,000,000.

The rocks of the region (arranging them according to the predominant feldspar in their constitution as made out by Mr. Becker) are as follows:

(1) *Having the feldspar portion chiefly orthoclase:* Granite, a small area near the Red Jacket mine; *quartz porphyry* (dacite of Zirkel), having a microlitic fluidal ground-mass. (2) *Having the predominant feldspar oligoclase: metamorphic diorite,* G = 2·96 (a variety of basalt, Zirkel) a blackish rock in the southern part of the district. (3) *Having the predominant feldspar labrado-*

*rite or andesine: eruptive diorite*, widely distributed, often quartziferous, having porphyritic and granitoid varieties, and some mica with the hornblende, the mica occasionally exceeding the hornblende in amount; *Diabase*, the *older*, forming the east or hanging wall of the lode, the *younger*, known as "black dike," extending horizontally more than a mile through some of the mines, and resembling the trap of New Jersey, neither of the kinds having a microlitic or glass-containing ground-mass; *andesite*, having a microlitic ground-mass and often a fluidal structure, including *earlier* and *later hornblende-andesite*, and also some *augite andesite* which differs mineralogically only in the proportion of augite, the *later* hornblende-andesite often called trachyte; and *basalt*, consisting chiefly of labradorite, augite and olivine.

Magnetite and titanite are present in most of the rocks; mica, in much of the diorite and some diabase; zircons, in the granite, quartz-porphyry, diorites, and rarely in the andesite; some hornblende, and rarely mica in the diabase. The granite and diorites are the older of the rocks. After these, the chronological order deduced is as follows: Quartz-porphyry, earlier diabase, later diabase, earlier hornblende-andesite, augite-andesite, later hornblende-andesite, basalt. The most abundant rocks are the andesites. The microscopic character of several of the rocks is illustrated on four plates of sections.

The "propylite" of von Richthofen was proved to be an altered rock, either andesite or diabase.

The rocks adjoining the lode are altered to great depths, and fresh specimens of several kinds are unobtainable. They are often chloritic from the change of augite, hornblende or mica; sometimes contain epidote, which is referred to the alteration of chlorite; calcite, from the lime of the feldspars and exotic $CO_2$; uralite, from the change of augite; and pyrite, from different sources, the iron coming from one of the iron-bearing silicates. Among the results of weathering, freezing is stated to throw off sometimes conchoidal chips from the angles of andesite blocks to a distance of two or three feet which weigh three or four pounds. The quartz porphyry is deeply decomposed, and its porous structure is said to be "perhaps due to the unequal contraction of the quartz and feldspar in cooling."

The changes along the lode are attributed to waters holding carbonic acid or alkaline sulphides, or other mineral ingredients. The conclusion is drawn, based on assays of the rocks, admitted to be not altogether satisfactory, that the diabase, making the east wall of the vein (and now exposed for a length of 8,000 feet and a thickness of 2,500 feet, and deeply decomposed), may have been a chief source of the gold and silver; that it is probable, from Professor Sandberger's investigations that the augite of the diabase was the seat of its metallic contents; and that sulphydric and carbonic acids were the active reagents that extracted and gathered in the ores of the metals. By their action on the rocks, "carbonates and alkaline sulphides would be formed, and

these are solvents for quartz and sulphides of the heavy metals. The heated depths of the mine are supposed to have formerly supplied sulphydric and carbonic acids for the decompositions and the filling of the vein, though now affording only a mere trace of these gases. The assays of the rocks adjoining the vein give mostly from 0·03 to 0·21 for the percentage of the precious metals reckoned as silver.

The theory sets aside any supply of ore-material from below except such as may have come up as a constituent of the diabase, and mostly of the augite of the diabase. The mine is on this view dependent at all depths for its value on the existence of the adjoining diabase, and hence "exploration," Mr. Becker observes, "should be confined to the neighborhood of this contact," and "should this rock narrow to a mere dike between diorite walls, the outlook would be gloomy."

This very simple theory has doubts about it because nothing is known of all that happened, in the course of the vein, from the time of the ejection of the diabase or the first opening of the fissure—when the temperature was that of the fused diabase—down to the time when it was 500° F. and less; nothing as to the nature of the rising vapors at this high temperature, when, because of the temperature and pressure, they would have great decomposing and carrying powers; nothing as to whether the wall of the great fissure may not, deep below, have contained metalliferous veins or beds, or disseminated ore, that would have yielded the metals or their ores easily to the powerful agents of extraction, those highly heated vapors; nothing as to whether these sources of supply may not have contributed to the diabase and other rocks the traces of precious metals indicated by the assays.

The heat of the mine is proved to increase downward, at the rate of about 3° F. for each additional hundred feet. "On the 3,000-foot level"—to which depth the country is honey-combed with passages—"floods of water have entered the mines at 170° F.," hot enough to cook food. The equation deduced from the facts gives for a temperature of 212° F. a depth of 5,200 feet; and "a boiling heat is likely to be struck at any time after passing the 4,000-foot level," and in all probability short of 5,000 feet.

The facts show that the source of the heat "can hardly be less than two miles from the surface, and is probably four, in short at

a volcanic distance. In the Sutro tunnel "the rise of temperature as the lode was approached is best expressed by a geometric ratio," showing that the heat is derived from the lode, the moisture or aqueous vapor present enabling the rocks to conduct heat.

In addition to the escaping aqueous vapor, about 7,000,000 tons of hot water are now yearly pumped from the lode, all of subterranean source but probably originating in the high country of the Sierra Nevada, as the preceding ideal section illustrates.

The question as to feldspar-decomposition (resulting in the making of kaolin) being the source of the heat, as urged by Church, is discussed, and made the subject of careful experiments by Dr. Barus; the conclusion reached is that the amount of heat thus derived is infinitesimal, or not enough to be detected by an apparatus sufficiently delicate to register 0·001° C. Further it is shown that the so-called clay or kaolin is chiefly pulverized rock.

Mr. Becker's Report gives details also with reference to the position of the lode and its contents, leaving much however to be presented to those interested by the fine maps and sections of the folio atlas. It treats also of the structural results of faulting. Besides, it gives the details of an electrical survey of the Comstock and Eureka mines, in which the former afforded only negative results, and the latter a slight action, perhaps enough to be of use in prospecting, especially where the ores are sulphides.

2. *U. S. Geological Survey: Field Work in the Great Basin.*—The division of the Survey to which the Great Basin was assigned, has been employed for several years in a study of the system of lakes which in Quaternary time occupied that desert region. During the past season the professional corps has consisted of Mr. G. K. Gilbert, geologist in charge; Mr. Israel C. Russell, assistant geologist; Mr. W. D. Johnson, topographer; and Ensign J. B. Bernadou, U. S. N., general assistant. Mr. R. Ellsworth Call of David City, Nebraska, is not permanently connected with the work, but has been and is still engaged in the study of the recent and Quaternary shells of the Great Basin, and he accompanied the field parties for a short time. Mr. Gilbert spent a month with Mr. Call in the basins of Humboldt and Pyramid Lakes, and then proceeded to the Mono Basin where the remainder of the party had been at work from the beginning of the season. Mr. Russell has had charge of the investigations in the Mono and Lahontan Basins, and has this year completed his field examinations. The chief part of the summer was spent in the Mono Basin including the adjacent eastern slope of the Sierra Nevada, and the most important subjects of study were the ancient expansion of Mono Lake, and the ancient glaciers of the Sierra. Some attention was also given to the ice-masses which still cling to the northern slopes of the higher peaks, and the camera will be made to give its evidence as to their title to the name of glacier.

All have now returned to the east except Mr. Johnson, who, having completed a general survey of the Mono Basin, remains to make topographic sketches of certain local features, especially of ancient moraines. The Survey has determined to discontinue this investigation of the Quaternary lakes of the Great Basin. The study of the most northern lakes is practically complete, but it has been found impracticable to carry the work southward at present, on

account of the great draft on the energies of the Survey incidental to the recent extension of its field. A preliminary report on Lake Bonneville was published in the Second Annual Report, and has been noticed in this Journal. The Third Annual Report will contain a preliminary essay, by Mr. Russell, on Lake Lahontan, and the Fourth, a brief description of the district north of Lake Lahontan, where there was a group of smaller lakes. These are now in type, and the preparation of the final memoirs is well advanced.

3. *Vol. VII of the Geology and Paleontology of Illinois.*—Volume VII of the series of Reports on the Geology and Paleontology of Illinois, A. H. WORTHEN, State geologist, has recently been issued. The first part, by Mr. Worthen, treats of the development of the coal resources of the State since the publication of the last volume in 1875, and covers 62 pages. The remainder of the volume, over 300 pages, is paleontological. Over sixty species of fossil fishes are described by O. St. JOHN and A. H. WORTHEN, and illustrated on twenty-six plates, crowded with figures. Descriptions and plates of many invertebrate fossils, mostly new crinoids, follow, Part I, by A. H. WORTHEN and S. A. MILLER, and a second part, by C. WACHSMUTH and W. H. BARRIS. The excellent figures are mostly from drawings by C. K. Worthen. Added to those of previous volumes, the whole number of plates issued in the Reports of the Illinois Survey is now 196. Two more volumes, containing forty to fifty plates each, will be required to complete the paleontology. The series of volumes has no rival in paleontological value among the State reports, if those of New York are excepted.

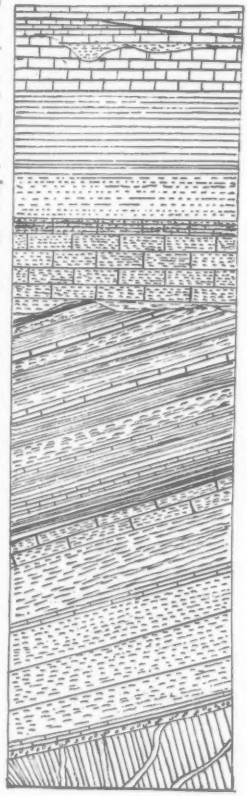
4. *On the recent formation of crystals of Cerussite.*—M. LACROIX has described the occurrence of several copper minerals in connection with pieces of Roman money found among some ruins in Algeria. The coins, as found, adhered to each other, being cemented together by carbonate of copper; between each pair there was a little cavity containing crystals of cerussite and cuprite, with malachite and azurite, but varying according to the degree of alteration which the coin had undergone. The cerussite was in small mammillary crystalline groups, of a yellowish-white color; the cuprite formed red cubes up to $1^{mm}$ in thickness; the azurite was also in a crystalline state, but without distinct form. The composition of the coins was obtained, as follows: Cu 79·76, Pb 16·26, Sn 3·97=99·99. Somewhat similar observations have been made by Daubrée on recent minerals occurring at Bourbonne-les-Bains, but in the latter case the cerussite was formed directly from fragments of lead, and not as here, at the expense of the lead in the coin alloy.—*Bull. Soc. Min.*, vi, 175.

5. *Volumes I and II of the Reports on the Geology of Wisconsin* have recently been issued. They are volumes of great geological importance. A notice of them is deferred.

6. *Supposed Glacial striæ on Locust Mountain, Pa.*—On page

473, of the last volume of this Journal, the observation that glacial striæ occurred on Locust Mountain, south of the glacier limit, is cited from remarks by Prof. Lesley in the geological report of Mr. I. C. White. We learn from Prof. H. C. Lewis, that he has made a special examination of the region and finds that the supposed glacial striæ are not of glacial origin.

7. *The Pre-Carboniferous Strata in Arizona;* by C. D. WALCOTT.—The following cut represents a section, by the author, of the Pre-Carboniferous strata in the Grand Cañon, in illustration of his article on pages 437–442. Owing to the dip of the strata, a portion only of the Pre-Tonto groups is shown.




8. *Notes on a new Topaz Locality;* by Rev. R. T. CROSS, Denver, Colorado. —In the October number of the Journal for 1882, Messrs. C. W. Cross and W. F. Hillebrand, of the U. S. Survey, described some topazes of unusual size found near Pike's Peak. The locality was in the disintegrated granite on one of the steep slopes about four miles from Manitou. Only a few crystals were found, less than half a dozen good ones, and perhaps twice that number of phenacites.

During the summer of 1883, Mr. Walter B. Smith, a collector, was hunting for minerals in the foothills twenty-five miles north of Pike's Peak, a region largely destitute of ores and hence not much traversed by prospectors, when he found, near Platte Mountain, a "pocket" of topaz crystals. After digging more or less for a month he secured over a hundred crystals and cleavage fragments. A large number have come into the possession of the writer. The pocket was found in decomposed albite. In the near vicinity is found pegmatite or graphic granite. The following minerals were found in the pocket or very near to it: micro-

cline, albite, clear and smoky quartz crystals, an altered mica, gœthite and fluorite. The largest microcline crystal found measured eighteen inches in the longest diameter. Many of the topaz crystals have been broken—being found out of place—but they show an attempt on the part of nature to heal over the broken surfaces by forming new planes. Some of the microcline crystals show the same peculiarity. The largest topaz fragment weighs eleven and one-half ounces and parts of it are very clear. It evidently belonged to a very large crystal, only two planes of which appear on the fragment. Another fragment, weighing two and a half ounces, is perfectly limpid and colorless. The largest and best crystal weighs four ounces. It is of a light straw color and is perfectly clear, having no flaw in it. The prism is perfect, showing the planes *I*, $i$-2, and $\breve{\imath}$-2. *O* is distinct, though slightly bruised. Domes, $\frac{3}{2}$, 1, $\frac{2}{3}$, 2-$\bar{\imath}$, 2-$\breve{\imath}$ (the latter passing probably into $\frac{4}{3}$-$\bar{\imath}$), 4-$\breve{\imath}$, and $\frac{2}{3}$-$\bar{\imath}$.

9. *Jeremejeffite and Eichwaldite.*—The chemical characters of the remarkable new borate of alumina from Siberia, called by M. Damour *jeremejeffiite*, have already been given in this Journal (xxv, 478). The crystalline form of the species has since been described by Websky. An optical examination by him has confirmed the observation of Jeremejef, made in 1869 upon the same crystals, which were then supposed to be beryl, viz: that only the outer shell is optically uniaxial, while the interior portion is made up of six segments, all alike biaxial. Websky found the two parts of the crystal to be separated by a narrow nearly opaque hexagonal ring. The boundary lines of the six interior segments are perpendicular to the prismatic planes. The optical characters of all the segments correspond, the bisectrix being parallel to the vertical axis, and the plane of the optic axes making an angle of 30° with the adjacent sides of the exterior hexagon. The two parts of the crystal are morphologically different, and to the uniaxial part Websky proposes to restrict the name of Damour, while to the interior part he gives the name *Eichwaldite*, from that of the Director of the mines at Nertschinsk, who collected the crystals. A study of the crystalline form has convinced Websky that the prismatic planes and the pyramidal terminations correspond to the pyramidal-hemihedral division of the hexagonal system, while on the rounded extremities there also occur planes which belong to the interior *Eichwaldite*, and which are to be referred to a *drilling* of the orthorhombic system in accordance with the optical character. It is to be regretted that the material is too scanty to allow of fully deciding these complex crystallographical relations, as also to show whether both parts of the crystals have the same composition. This indeed seems probable, and it is natural to think of the well-known cases in which the same chemical substance is known to appear in both the hexagonal and orthorhombic (or monoclinic) forms; the observations of Professor Cooke on the iodides of antimony are to the point.—*Ber. Ak. Berlin*, June 14, 1883.

10. *Mineralogical Notes;* by E. CLAASSEN. (Communicated). (1) *On interesting crystals of Pyrite.*—These crystals occur in a bluish-gray shale, the so-called Cuyahoga shale of the Waverly group, at Parma, Cuyahoga county, Ohio. They form combinations of the cube, octahedron and generally the pyritohedron ($\alpha$O2), with the cube predominating. The largest crystals found measure about $\frac{1}{4}$ inch in diameter. The interest of these crystals consists in the fact that the cubic planes have always a concave surface, while the planes of the other forms are as even as usual. In consequence of this fact the combination-edges of the cube and octahedron are often curved lines. A single crystal among those examined was peculiar in that the octahedral planes consisted of a depressed triangular portion with an even surface, bounded by three narrow raised parts; the cubic planes have, with one exception, a concave surface; the pyritohedral planes are divided into two parts by a narrow depression. It seems probable that the planes of the original crystal, in this and other cases, were even, and that the concavity and other irregularities observed are due to subsequent growth of the crystal over this nucleus.

(2) *On Magnetite-crystals in Hematite.*—Small perfect crystals of magnetite are found imbedded in a compact hematite of metallic luster, brought from the Lake Superior Iron Mines. They are octahedrons, have a red streak and give a red powder exactly like the surrounding hematite, but differ essentially by their being attracted by the magnet. Their appearance is that of unaltered magnetite and not that of martite, which often also occurs in the above named region.

(3) *Vanadium and Titanium in Magnetite in the so-called Chaffey ore.*—This magnetic iron occurs near Newboro, Canada, and contains $\frac{1}{36}$ per cent of vanadic acid and $9\frac{1}{4}$ per cent of titanic acid. The vanadic acid was determined in a quantity of five pounds of pulverized and finely bolted ore, and separated as barium resp. ammonium vanadate.

11. *Phytogeogenesis: Die vorweltliche Entwickelung der Erdkruste und der Pflanzen in Grundzügen; dargestellt* von Dr. OTTO KUNZE. Leipzig, 1884. pp. 213, 8vo.—This curious treatise, on the original development of the earth's crust and its vegetation, is of a highly hypothetical character. Much of it we must leave for the geologist to characterize and to digest, as best he may; for instance, that our globe was never an incandescent liquid, nor melting hot, but only moderately red-hot; the oceans formed after sufficient cooling were strongly impregnated with carbonic acid in the form of bicarbonate of calcium, but scarcely salt. The gradual disintegration of the land surface gradually gave the sea its saltness. The vegetation which flourished on the surface of this highly carbonated sea gave to the air its stock of carbonic acid. All vegetation down to a comparatively late period—mainly down even to the Tertiary—was aquatic and marine; the vegetation of the Carboniferous period, the Lepidodendra and other higher cryptogams, formed floating islands of vegetation, their

Stigmaria roots floating on the surface in matted masses, their stems rising like masts into the air. As the ocean grew salter this Carboniferous vegetation took to the strand; and then, or subsequently, the vegetation of the world was in peril and verged to extinction, as may be seen from the fact that there were 2500 species of coal plants, and only 150 in later periods. It would seem that increasing saltness of the ocean had driven the higher cryptogamic vegetation to the land before the continents were prepared to receive it; but finally, when the atmosphere had acquired from the bicarbonated ocean sufficient carbonic acid and the rocks had worn down into soil, terrestrial vegetation was established. In the ninth period, heretofore called the Tertiary period, land-plants came to prevail, and the differentiation of climates gives rise to differentiation of land vegetation. Man now appears, in all races probably first with black skin. As there are no indigenous blacks in America, the early population of this continent was an immigration from the old continents. To go back to the beginning: the earliest living things, *Protista*, neither animals nor plants distinctively, were neither miraculously created nor imported from other worlds; but the primordial cells were formed by a kind of mechanical precipitation, in a way which Traube's artificial cells explain. Life arose from certain catalytic processes in carbo-hydrates: only some one of these, with tannin or something of the sort which may precipitate it from a solution, seem to be necessary to cell-formation and growth, and consequently for propagation by division. Sexual propagation in the first instance was probably a morbid process, a sort of diseased action. The two or three primordial organic carbo-hydrates needful to life originated from certain inorganic carbo-hydrates of the earth, and these, from interplanetary space. Dr. Kunze is a botanical writer of no small pretension and learning, and is fond of treating his topics speculatively. It may suffice to call attention to his volume, which is replete with suggestion.

A. G.

12. *Catalogue of the Phænogamous and Vascular Cryptogamous Plants of Worcester Co., Massachusetts;* by JOSEPH JACKSON. Worcester. Published by the Worcester Natural History Society, 1883. pp. 48, 8vo.—"Worcester County extends across the State from North to South. On its southern or Rhode Island margin it is allied by its flora to Southern New England; on its northern or New Hampshire margin it is allied to northern New England. It lies at altitudes varying from 200 feet to 2,500 feet above the sea level. Only one point, Mount Wachusett, reaches the latter height. . . . It is a thickly wooded region, with many kinds of valuable timber trees, such as the white pine, white ash, white oak, hickory, chestnut, rock maple," etc.

A faithful list of the plants of such a typical slice of New England, besides its local uses, is interesting and instructive, as giving a fair idea of New England vegetation apart from the influence of the sea on the one hand and the alpine and subalpine

heights on the other. "No attempt has been made to distinguish between indigenous and introduced plants,"—which is rather to be regretted from our point of view. One likes to see the native as clearly distinguished from the foreign elements as well may be. For such a district this may now be done quite clearly. In the future it will be more difficult. The Catalogue "contains 812 species and well-marked varieties, distributed among 387 genera;" and "625 species are found in Millbury," the author's own residence. The Catalogue is well edited and handsomely printed. We have detected only a single misprint (and that on the last page), besides the wrong use of a capital initial in one instance, which probably is also an error of the press,—a correctness rarely attained in a first essay. There is a list at the end of the "trees, shrubs, and evergreen flowering plants growing naturally in the county, 158 in number; also of twelve trees, none which are common in cultivation." A. G.

## III. Miscellaneous Scientific Intelligence.

1. *The Ice of Greenland and the Antarctic.*—Dr. James Croll has a paper, in the Philosophical Magazine for November, opposing the view that the universal ice-covering of Greenland is a consequence of the elevation of the land. He states that no interior mountain ranges have been reported, and holds, as urged by Dr. Robert Brown, "one of the highest authorities in matters relating to Greenland," that the region of the great interior ice-field is like a broad-lipped shallow vessel, filled with the glacier as with a viscous material, and pouring portions out through breaks in the margin. He also accepts as most probable the opinion, long since expressed by Giesecke, and more recently by Dr. Brown, that the country is really a collection of islands fused together by ice.

Dr. Croll also argues that the Greenland condition is probably that of the Antarctic—a collection of rather low islands "bound together by a continuous sheet of ice," as Sir Wyville Thomson has said, covering a space of about 4,500,000 square miles. This conclusion as to its little elevation is stated to follow from the low and even top, and the structure, of the Antarctic ice-barrier. Its horizontal stratification-bands indicate, by their number and the thickness of the mass, a long period of annual deposition of snows; and also that the barrier is probably removed hundreds of miles from the region of dispersion, especially since there are none of the usual marks of a former glacier-condition, such as come from movement along valleys or fiords. The layers constituting the ice-mass become gradually thinner downward; and this, while partly a consequence of compression and melting, is due more, Dr. Croll says, to the increasing breadth of the earth southward from the pole, this alone requiring that a square foot of ice coming from latitude 89° should occupy thirty square feet on reaching latitude 60°, and hence be only $\frac{1}{30}$th of a foot thick.

Hence relative thickness is an indication of distance traveled, and "a layer near the bottom may have been traveling from the Pole for the past 10,000 or 15,000 years," while one at top is not twenty years old, and of short distance of travel.

From such facts Dr. Croll argues that the ice of the Antarctic ice-barrier moved off from a low and even land-surface, and that this kind of surface characterizes the "Antarctic continent." On the ground that the moist winds flow in all directions toward the south pole, and that the latitudinal extent diminishes poleward, it is urged that the loss by precipitation on going poleward does not necessarily occasion a diminishing depth of ice. For if the annual amount of precipitation between the parallels of 60° and 80° is thirty feet, and the amount of moisture left in the air on reaching the line of 80° is only a tenth of thirty, this tenth would give a snowfall there of twenty-four inches, because the area between the line of 80° and the pole is only an eighth of that between the parallels of 60° and 80°. Hence even if the border of the Antarctic ice-region takes from the clouds the greater part of their moisture, the thickness of the ice may still increase poleward. Moreover the region of the pole should be ever filling with ice, however small the precipitation.

Dr. Croll regards the recent observations of Nordenskiöld as confirmatory of his views on Arctic and Antarctic ice-regions.

2. *National Academy of Sciences.*—At the meeting of the Academy held at New Haven, November 13–16, 1883, Professor O. C. Marsh, President, the following papers were presented:

A. GRAHAM BELL: Upon the formation of a deaf variety of the human race.

C. A. YOUNG, E. S. HOLDEN and C. S. HASTINGS: On the Solar eclipse of May 6th, 1883.

ASAPH HALL: Notes on the mass of Saturn.

R. H. CHITTENDEN: On some new primary cleavage forms of albuminous matter. *By invitation.*

S. NEWCOMB: On the use of the word "Light" in Physics; On the theory of errors of observation and probable results.

O. T. SHERMAN: Personality in the measures of Venus's diameter as derived during transit across the Sun. *By invitation.*

E. LOOMIS: The reduction of barometric observations to sea-level.

S. P. LANGLEY: Atmospheric absorption.

W. H. BREWER: On the subsidence of particles in liquids.

H. A. ROWLAND: On a new photograph of the Solar Spectrum.

J. D. DANA: On the stratified drift or terrace formation of the New Haven region, including its kettle holes and deserted river channels.

W. GIBBS: Preliminary notice of phospho-vanadates, arsenio-vanadates, and antimonio-vanadates; On the probable existence of new acids of phosphorus.

B. SILLIMAN: Notes on the mineralogy and lithology of the Bodie Mining District of California.

J. S. NEWBERRY: On the ancient glaciation of North America.

J. W. POWELL: Marriage Institutions in Tribal Society.

J. HALL: Note upon the physical aspects of the higher members of the Chemung Group and the development and distribution of the fossil genera Ptychopteria and Leptodesma, preceded by a review of the Pectenidæ and Aviculidæ of the Devonian system.

T. STERRY HUNT: The Animikie rocks of Lake Superior.

O. C. MARSH: On the affinities of the Dinosaurian Reptiles.

3. *Signal Service Professional Papers.*—The following papers have been issued by the Signal Service. No. VIII. The motions of fluids and solids on the earth's surface, by Professor WM. FERREL, reprinted with notes by Frank Waldo.—No. IX. Charts and Tables showing geographical distribution of rain-fall in the United States, based on observations from the establishment of the Meteorological Bureau of the Signal Service, in 1870 to January, 1881, by H. H. C. DUNWOODY, 1st Lieut. 4th Artillery, acting signal officer.—No. XI. Meteorological and Physical Observations on the East Coast of British America, by O. T. SHERMAN.—No. XII. Popular Essays on the Movements of the Atmosphere, by Professor WM. FERREL.

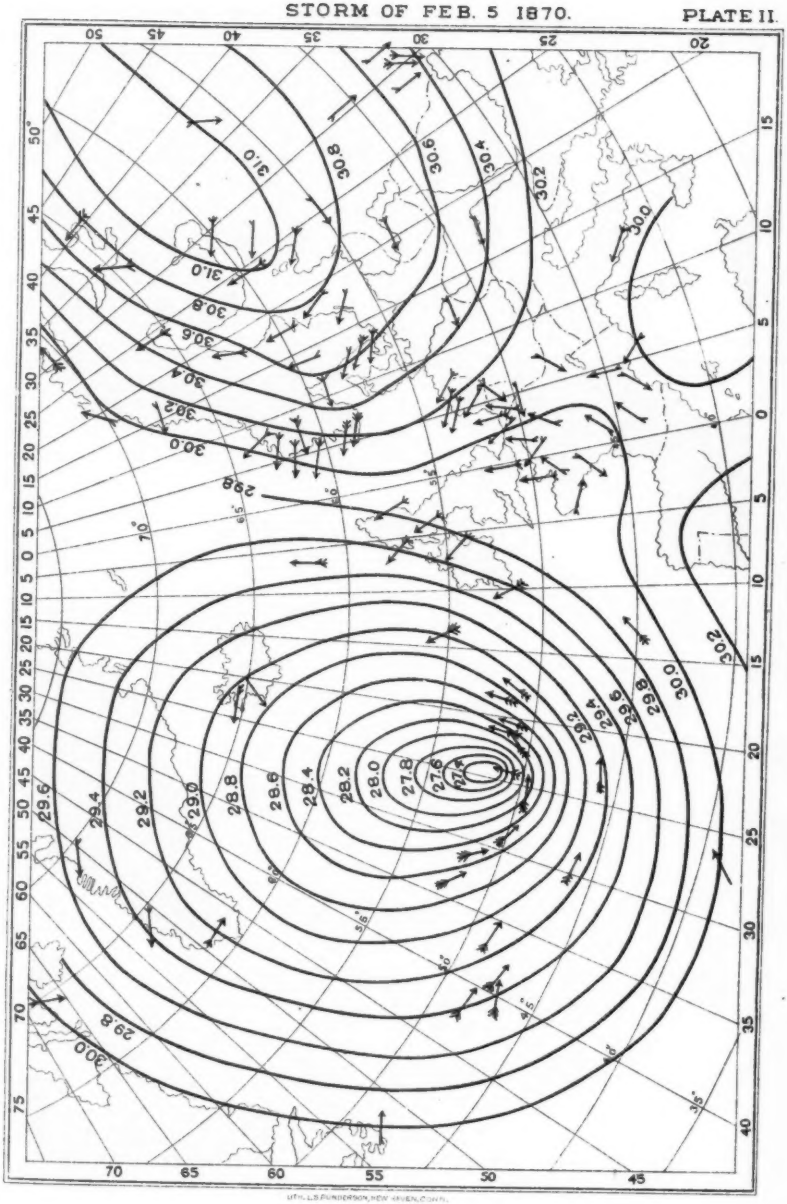
## OBITUARY.

JOHN LAWRENCE LECONTE.—Doctor John L. LeConte, the able Entomologist, died on the 15th of November, at Philadelphia, in his fifty-ninth year, having been born in New York on the 18th of May, 1825. He was a son of Major John E. LeConte, a distinguished member of the Engineer Corps and the author of several papers on Botany and Zoology. Doctor LeConte's labors in science were devoted especially to the Coleoptera, in which department he made very large collections, and was high authority. He is the author also of some mineralogical papers, and as early as 1847 presented to the Association of American Geologists and Naturalists a valuable paper on Fossil Mammalia from Illinois. The Smithsonian Institution published in 1861–62 his *Classification of the Coleoptera of North America*, and in 1863–66 his *List of the Coleoptera of North America.* He was a member of the National Academy of Sciences, and, for the year 1873–74, President of the American Association.

LEONARD D. GALE.—Dr. Leonard D. Gale died in Washington on October 22, in his eighty-fourth year. He was graduated from the College of Physicians and Surgeons, in New York City, in 1830, and was soon afterward employed as Assistant Professor of Chemistry in that institution. His name is connected with chemical papers in this Journal for 1831 and 1832. Later he was Professor of Chemistry in the New York College of Pharmacy, and Professor of Chemistry, Geology, and Mineralogy in the University of New York. While holding the latter position, in 1834, he rendered important aid to his fellow-Professor, S. F. B. Morse, in the perfection of the electric telegraph, bringing to his knowledge the discoveries of Professor Henry, the application of which to the Morse machine assured its success. In 1838, Dr. Gale made a geological survey of New York island, and a report of his work is published in the New York Geological Report (4to, 1843) of Professor Mather. From 1846 to 1857 he was an Examiner of Patents, having charge of the Department of Chemistry, and was afterward for many years engaged in the practice of patent law in Washington.

BAROMETRIC GRADIENT PLATE I.

STORM OF FEB. 5 1870. PLATE II.



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ERRATA.

Page 177, line 4 from bottom, add at the end of the sentence, except the violin family.

Page 179, line 1 from top, for $2Vh$, read $2V_h$.

Page 181, equation (13), all the coefficients should be 2. Equation (14), all the signs should be alike, viz., +.

Page 183, line 17 from bottom, for These familiar, read Then familiar. Line 8 from bottom, after ù insert a comma, and dele comma after (ü).

Page 185, line 14 from top, after velocity add, of the tip of the prong.

Page 187, Table V, column 6, for 2·5, read 1·3.

Page 189, last line, for 1862, read 1882.

Page 217, Figure 5 represents a crystal of allanite, and was inserted by mistake.

Page 279, line 18 from top, for pyramids, read fragments.

Page 286, line 4 from top, before oxygen dele the.

Page 290, line 17 from bottom, for decomposition, read deposition.

Page 306, line 12 from bottom, insert vol. xxiii at the beginning of the line.

Page 307, line 11 from bottom, for focus, read locus.